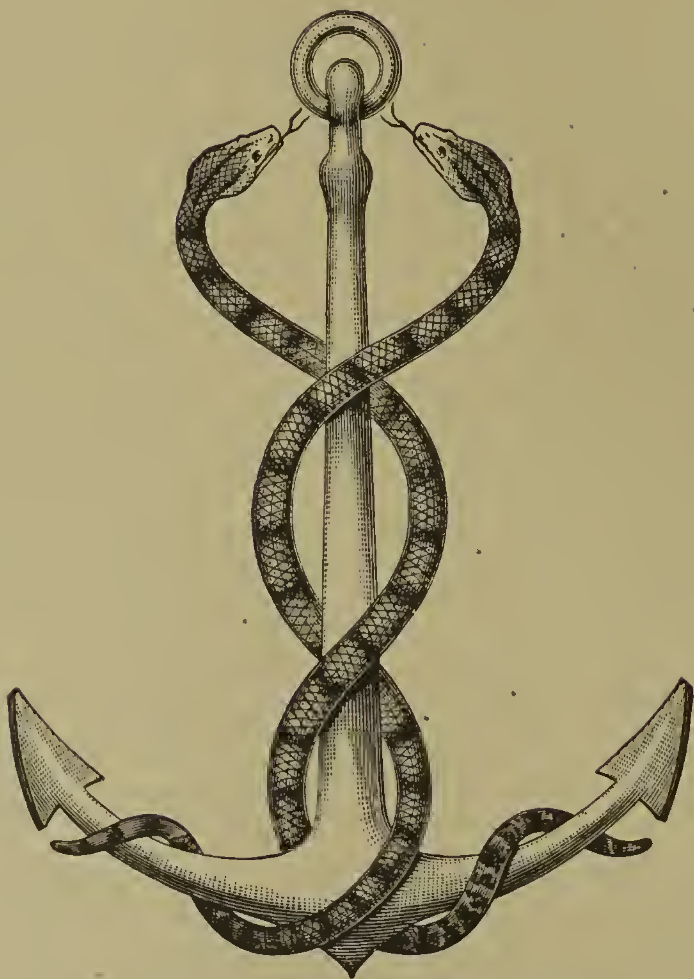
W. A. Herdman

NATURAL SCIENCE



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NATURAL SCIENCE

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NATURAL SCIENCE

25
Cloth to press

Natural Science

A Monthly Review of Scientific Progress

July 1899

NOTES AND COMMENTS.

The Animal Mind.

In the June number of *Natural Science* we had occasion to remark that comparative psychology is the most anarchic department within the naturalist's province. This is due to several causes: in part to the fact that, as we said, this field is often a happy hunting-ground for the crank, in part to a lamentable want of agreement in the use of psychological terms, and in part to the lack of any co-ordinated body of critical and adequately-trained opinion on the subject. The average press critique of a work on the instincts and intelligence of animals reveals the fact that there are comparatively few men to whom an editor can appeal with confidence that they have a sufficient background of knowledge to enable them to realise the true nature of the problems which are discussed. The more popular and superficial the interpretation in a work under review, and the more closely it accords with the current prejudices of those who, without special study, think they understand, not only mental products, but (a far more difficult matter) the subtle processes by which they are reached, the more likely is it to be hailed as the expression of the "plain common sense view of the question."

Two articles are devoted to comparative psychology in the May number of the *Psychological Review*: one by Prof. Wesley Mills on "The Nature of Animal Intelligence, and the Methods of investigating it"; the other by Prof. E. Thorndike on "The Instinctive Reaction of Young Chicks." The main object of the former writer is to criticise some of the previous work of the latter. The monograph by Prof. Thorndike thus criticised was reviewed in these pages by Prof. Lloyd Morgan, who urged, *inter alia*, that the method adopted by its author, that of placing starving cats in cramped cages, was unsatisfactory. This, too, is the burden of much of Prof. Wesley Mills' criticism. And so far he is on safe ground—ground which, as an independent observer, he knows well. But when he deals with psychological criticism the plane of his analysis is so different from Prof. Thorndike's, that little of value comes out of his discussion. He will, we think, enlist the sympathies of the uninstructed, rather than those of serious students of

psychology. He has himself published observations of interest and value—modestly asserting that he "has recorded more experiments (not to mention scores which he has not described) than all other investigators together, if we except those working on insects." But in analysis and interpretation he has not shown himself strong. It is questionable whether his discussion of imitation and memory, for example, have any real bearing upon Prof. Thorndike's contentions. Indeed, at one point he seems to dimly realise this, for he says: "To be sure, there is a sort of deliberate, studied, high-class imitation possible to man, but beyond the reach of animals." But he does not appear to grasp the fact that it is just the occurrence in animals (save, perhaps, the Primates) of such imitation which Prof. Thorndike questions. Speaking of "free floating ideas," Prof. Mills says: "The believer in evolution will demand that, in this and other cases, in which qualities man possesses are denied to animals, there be the clearest proofs given. The burden of proof lies with those who deny them." With this assertion many psychologists entirely disagree; and Prof. Wesley Mills' *ipse dixit,* without adequate discussion, will not lead them, we imagine, to alter their opinion. It is strange that Prof. Lloyd Morgan's name should be mentioned as that of one who holds the view "that we must always adopt the *simplest* explanations of an animal's action," seeing that in his "Introduction to Comparative Psychology" (p. 54), he urges that the simplest explanation is *not* that which we should necessarily accept.

Prof. Thorndike's article deals with young chicks. His observations tend, on the whole, to confirm those of previous investigators, but add some interesting facts. The newly-hatched birds were found to peck at small (2 mm.) squares of coloured paper on backgrounds of white and black. The observations are not sufficient in number to justify conclusions as to colour preference; but they suffice to establish the fact that the patches, either from their colour or their light intensity, afford the requisite stimulus to the pecking response. Mr. Thorndike found that chicks from ten to twenty days old ate bees greedily, "first mashing them down on the ground violently in a rather dextrous manner." It is probable, however, that they would not have touched them had they been stung then or at an earlier stage in their experience. He makes a point here against Prof. Lloyd Morgan, who states that a young bird dropped a bee, shook his head, and wiped his bill on the ground, "probably because he had tasted the poison." This statement, indeed, hardly seems to accord with Lloyd Morgan's own later observations of the eating of wasps and bees by young birds of several kinds. Other noteworthy facts which Prof. Thorndike records, are that young chicks placed in water will swim, and that, prior to experience, they will not leap down from a height of 39 inches, though they will do so at once from a height of 10 inches or less, and after some hesitation from heights of 16, 22, and 27 inches. In general

Prof. Thorndike thinks that too much stress has been laid on the definiteness of instinctive response, saying that the same stimulus does not always produce exactly the same effect in all individuals. But much depends on the meaning of the phrase "the same stimulus." It is at least possible that some part of the difference in response is due to slight difference in the stimuli and the "situation." But there are, no doubt, also differences in the individual characters of the birds (as all observers will be ready to admit) which lead to divergences of behaviour under quite similar circumstances. In any case the observations which Mr. Thorndike here describes were well worth placing on record.

The Art of Self-Defence.

In the struggle for existence plants have specialised along the line of passive resistance. It is by this method, as Professor Stahl showed long since, in his famous essay on "Pflanzen und Schnecken," that many are saved from snails whose appetite is spoiled by the bitters and alkaloids which many plants contain, and in half a dozen other ways. Dr. Bokorny has worked out the same idea in reference to fungi, pointing out that there are many common vegetable substances which are almost fungus-proof, and that is saying a good deal. In his essay (*Biol. Centralbl.* xix. 1899, pp. 177-185) he shows how the self-preservation of plants against fungi is secured by stuffs like tannin, oxalic acid, ethereal oils, the lupulit of hops, and so on. He gives his thesis greater solidity by a table of the more important vegetable substances, their occurrence, and their relation to bacteria and other fungi. It must of course be borne in mind that this indication of a secondary advantage should in no wise be allowed to make us more sluggish in trying to find out the primary physiological import of these results of metabolism.

Trustees of the British Museum.

No one need quarrel with the latest election to the Honourable Board of Trustees of the British Museum, in the places of the late Baron Ferdinand Rothschild and Charles Drury Fortnum. The Hon. Walter Rothschild is a keen zoologist on a spacious plan, and one who has never allowed the interests of his own admirably worked museum at Tring to conflict with the friendship he so frequently displays for the Natural History Museum. Sir Henry Howorth is known to our readers, not merely as a learned historian of human and pre-human times, but as an enthusiast on matters of museum arrangement and equipment. A little keenness is a welcome leaven in a body of men appointed for the most part for any reason other than interest in museum matters.

The Scaly Squid.

SOME four years ago Professor Joubin of Rennes astonished the scientific world by the announcement that the Prince of Monaco had obtained from the stomach of a sperm whale the trunk of a large cephalopod covered with scales. Some light has been thrown upon this curious structure in a recent paper by Dr. Einar Lönnberg in the results of "Svenska expeditionen till Magellansländern." This author describes a very complete example of *Onychoteuthis ingens*, in which the pallial surface had a peculiar warty appearance. In transverse section there were visible, between the muscular layers and the epidermis, large flat papillae, some 4 mm. in diameter by 1 mm. thick. In the spirit specimens the skin had sunk down between the papillae, giving the surface of the body the appearance of irregular tiles paving an old-fashioned street. On microscopic examination each papilla is found to be made up of a parenchymatous-looking mass of connective tissue. Dr. Lönnberg points out that if the integument were removed, as had been done in Joubin's specimen by the digestive action of the cachalot, the papillae would present the appearance of the scales described by that author. Regarding the function of this organ Lönnberg suggests that it may be an adaptation "to hydrostatic pressure when the animals descend to great depths;" and he mentions that a gelatinous subcutaneous structure has been observed in other deep-sea cephalopods, such as *Alloposus* by Joubin, and in large species of *Ommastrephes* by Steenstrup.

Echinoderms at the British Museum.

UNDER the new Director, additions and improvements continue to be introduced at the Natural History Museum, London, with no less rapidity than in the days of Sir William Flower. The gallery devoted to recent echinoderms and worms, which groups are in the hands of Mr. F. J. Bell, has for some little time been changing for the better. Several examples of the softer-bodied forms, such as cannot be displayed in the dry state, are now beautifully mounted in spirit, while, in the case of the holothurians, the form and colouring of the living animal is shown by a series of sketches of the living holothurian of Ceylon, prepared under the direction of the late Dr. Ondaatje. There are two charming water-colours, we believe by Mr. C. Berjeau, of the rosy feather-star and the holothurian, *Cucumaria crocea*. Similar drawings adorn the coral gallery, and are a vast improvement on the usual class of wall-diagrams one sees in museums. Dried holothurians are not forgotten, for, as every schoolboy knows, they form an important article of diet in the far East under the name of Trepang;

and we have heard that it is proposed to devote rather more attention to the economic aspects of zoology than has hitherto been the custom at the Natural History Museum. Consequently the seeker after new delicacies can now see in this gallery a series of specimens of Trepang, purchased in the Canton fish-market, and presented by George Tradescant Lay, Esq.; he can learn their zoological and their vernacular names, the character of the food afforded by each, and the market price. A table-case with dark red velvet ground and buff labels (not unlike those in the U. S. National Museum) is a pleasing experiment in museum-installation, and undoubtedly shows off the tests of sea-urchins and star-fish to great advantage. Some exceedingly choice specimens are mounted under glass shades fixed on the table-cases. There are *Diadema saxatile*, a sea-urchin with unpleasantly long spines, presented by Dr. J. Anderson; two finely preserved brittle-stars, *Pectinura maculata*, brought from New Zealand by H.M.S. *Challenger*; and a monster *Echinus esculentus* from Plymouth, presented by C. Stewart, Esq.

Accessions to the Natural History Museum.

An innovation that is of practical value, and that should increase the interest of the public, is the assignment of one of the alcoves in the central hall of the Natural History Museum to the exhibition of specimens recently acquired. In this way those familiar with the Museum are less likely to overlook important accessions in the vast mass of accumulated material, while those whose familiarity is less than it should be will have their sluggish interest aroused by the mere statement that what they are looking at is "something new," for in this respect all men are Athenians. Hitherto the exhibits in this alcove have been confined to zoological specimens, perhaps because the Director is also keeper of the Zoological Department. The following have been on view: Fish, mollusca, and other invertebrata, from Lake Tanganyika, collected by Mr. J. E. S. Moore, illustrating the marine origin of the fauna and its antique character. Fish from the river Congo, described by Mr. Boulenger (*Annales Mus. Congo*), and presented by the Secretary of State of the Congo Free State. *Lepidosiren paradoxa*, collected in the Paraguayan Chaco by Mr. J. Graham Kerr. A collection of rare birds from Patagonia and Argentina, presented by Dr. F. P. Moreno, director of the La Plata Museum. The splendid Hexactinellid sponges from Japan, to which we have previously referred. A male *Cervus sica manchuricus* in full summer coat,—a splendid specimen, presented by the Duke of Bedford. And a large specimen of the Tarpon fish, *Megalops thrissoides*, captured off Florida by Mr. Otis A. Mygatt, and presented by H.R.H. the Prince of Wales.

Bryozoa and Bipolarity.

Sir John Murray may take heart again. His attempt to explain the similarity between the north and south temperate faunas has been met by more that one specialist (even among those quoted in support of his argument) by a denial of the similarity, at all events to the extent assumed by the bipolar hypothesis. But now comes a lady to defend the knight. Miss Edith M. Pratt, of Owens College, Manchester, has been studying some collections, chiefly of Bryozoa, made on the shore of the Falkland Islands (*Manchester Memoirs*, vol. xlii. No. 13, 14th December, 1898). After a careful analysis of the distribution of the genera, she concludes that the results "*as far as Bryozoa are concerned*, seem to support Murray's theory." "Each genus represented in the collection occurs fossil, and also occurs in the north and south temperate zones, as well as in the tropics; in fact most of the genera are cosmopolitan. Many of the *species* are represented in the *Tertiary deposits*. This shows that the changes of climate and the altered conditions of life have not affected their 'Tertiary' structure; as many of these forms occur only in the two temperate zones, there is reason to believe that they have retained their common ancestral structure. The fact of many of the species occurring in the deep sea hardly supports Ortmann's theory [that an exchange of polar forms can take place through the deep sea], for many of them occur at very great depths only in the temperate regions; in the *tropics* they occur in *shallow* water. Their presence in the deep sea is, I think, the result of accident."

It is pleasing to find some attention paid to distribution in former geological periods; but does Miss Pratt, or can Sir John Murray, suppose that what took place in Tertiary times has much bearing on the question? It cannot seriously be maintained that there was any appreciable difference of world-temperature so recently as the Tertiary; certainly there was no approach to a universal climate in those days. We have to go back a good deal farther before our facts can bear any relation to the primal temperature of the globe. If there be a similarity between the present polar faunas, we do not see how any identity of species can be due to events that took place, if at all, in early Palaeozoic ages. As for certain cases of distribution being "the result of accident," what can Miss Pratt mean? It is too easy a way of explaining inconvenient facts.

Miss Pratt also studies the distribution of Anthomedusae, Porifera, Polychaeta, Gephyrea, Mollusca, Echinoderma, Crustacea, and Tunicata. Out of twenty-four species, three have been recorded from north and south temperate regions only; one from north and south temperate regions and the tropics; one from tropics and southern hemisphere; and all the rest from the southern hemisphere only. These facts scarcely show a striking similarity between the temperate faunas of the

northern and southern hemispheres. But, whatever conclusions may be drawn, the paper at least is one that does credit to the Zoological Laboratory of Owens College.

More about "Bipolarity."

Dr. Arnold E. Ortmann of Princeton, who pointed out in 1894 that the facts in regard to the distribution of Crustacea did not fit in with the "Bipolarity hypothesis," has some further remarks to make on the subject. He has been waiting, he says, for some definite expression of results from those who have been working at the "Hamburger Magelhaensischen Sammelreise," and he is disappointed. Perhaps Dr. Pfeffer's lecture at the annual meeting of the German Zoological Society—which he has promised to send us as soon as possible—may afford further light on the problem to which we recently referred in our summary of Professor D'Arcy Thompson's paper. The *onus probandi* seems to lie with the upholders of the hypothesis, but we wish that Dr. Ortmann would send us something more satisfactory than his recent note (*Zool. Anzeig.* xxii. 1899, pp. 214-216), which makes only one point, namely, that seven authors who have recently dealt with the question are all on his side. It seems absurd to lose good-humour on such a question, and even if Dr. Ortmann feels that he has ground for irritation it is a mistake to make this apparent. The proper safety-valve is an article in *Natural Science.*

Natural Science in Australia.

The Report of the seventh meeting of the Australasian Association for the Advancement of Science is a bulky volume of 1160 pages, which is full of interesting material, and affords abundant evidence of the activity of scientific life in Australia. The President, Professor A. Liversidge, who also edits the Report, dealt in his address mainly with some of the recent advances in physics and chemistry. Among the reports and papers more especially bearing upon natural science, we may notice those on glacial boulders in Central Australia, and on vernacular names of Australian birds; Captain Hutton's address on Early Life on the Earth (previously referred to in our columns); Dr. C. J. Martin's address on the history of the relations between morphology and physiology during the last fifty years; Mr. F. Manson Bailey's "few words" on the flora of the Torres Straits; Mr. J. F. Bailey's beautifully-illustrated paper on the plants of the rabbit-infested country in the Bulloo River district; Mr. A. J. Campbell's memoir on the

nests and eggs of the honey-eaters; Mr. W. J. Rainbow's observations on the long range of vision in spiders of the families Citigradae and Attidae. But this gives a mere hint of the interest of the volume. The president notes that the length of the journey often involved in a visit to a meeting of the Association necessarily tells on the attendance of members, and has led to the substitution of biennial for annual sessions, and he counsels the establishment of local scientific societies which would tend to increase the roll of working members. At the same time, that the plan of meeting biennially is a success as regards quality is evident from the stimulating and wholesome contents of this Report.

The Colouring Matter of Blue Coral.

PROF. LIVERSIDGE has made a series of experiments on the blue pigment of *Heliopora coerulea* on material obtained by the Funafuti Expedition. His results are interesting, although they do not, unfortunately, throw much light upon the nature or relations of this very curious pigment. He finds that "dead" coral after treatment with hydrochloric acid yields a black pigment which dissolves in formic, acetic, and lactic acids to form a bright blue solution. The pigment is slightly soluble in absolute alcohol, but quite insoluble in ether. The residue after ignition is bulky, and contains much phosphoric acid, iron, lime, and magnesia. Curiously enough Prof. Liversidge found that pieces of "live" coral, or coral which had been gathered while growing, although of a distinct slaty blue colour, did not yield blue solutions, but merely pale green ones. The pigment itself was also of a pale chlorophyll green tint. The paper concludes with a list of other blue or green colouring matters in animals. In connection with these we would draw the author's attention to the asserted occurrence of the mineral vivianite in the skeleton of *Belone* and some other forms.

Zoology in Brazil.

THE December number of the *Boletin* of the Pará Museum bears witness to the continued energy of the zoologists and botanists attached to that institution; the greater portion of the present issue being (as has so frequently been the case with its predecessors) from the pen of the learned director, Dr. E. Goeldi. Perhaps the most important item in the fasciculus is the article on the fishes of Amazonia and the Guianas, in the course of which a number of new species recently described by Mr. Boulenger are referred to. And attention may here be specially directed to the exceeding excellence of

execution and beauty of coloration characterising the double plate by which this article is illustrated. Our only regret is to find no mention of the habits of the various species of fishes referred to, although there is not improbably a sufficient reason for the omission.

That the habits of animals are not overlooked is amply demonstrated in the article headed "A Senda Amazonica du 'Caure.'" This deals with a beautiful little kind of nest, containing a single egg, which had long been attributed to *Falco rufigularis*, the "Caure" of the Brazilians. Struck with its resemblance to the nest of the Oriental *Collocalia nidifica*, Dr. Goeldi came, however, to the conclusion that it must be the work of one of the Tree-Swifts. And actual observation has proved the truth of the conjecture; the real builder being *Panyptila cayanensis*.

Other articles deal with the natives of Brazil, with the spiders of the country, and with the flora of Amazonia. A plate illustrating two species of monkey belongs to an article issued with an earlier part.

According to the Fancy of the Speller.

An attempt has often been made by embryologists to distinguish between those processes of development which appear to express an adherence to the mode established in the long evolution of a race (palingenetic processes), and those which appear to express readjustments or new departures adapted to conditions of relatively more recent date (kainogenetic processes). Thus it might be said that the development of a paired (epiphysial) upgrowth from the fore-brain was a palingenetic process, while the particular fate of these upgrowths or of one of them (which is very diverse in different types) is kainogenetic. There are some to whom the distinction seems of paramount importance, there are others who deny its legitimacy altogether, while a third position is that of those who recognise the distinction as an attempt to discriminate the relative age of the establishment of a developmental process, but find it exceedingly difficult to establish this in practical detail.

But, supposing the distinction be admitted as legitimate, what is its proper terminology? Keeping to the one root, καινός = new, we find, as Dr. Ernst Mehnert points out (*Anat. Anzeig.* xvi. 1899, pp. 29-31), cenogenesis, kenogenesis, cenegenie, caenogenese, and cänogenese. Our acumen is not sufficiently specialised to distinguish between the last two, but what about the others, in regard to which Mehnert writes, in response to the hot irons of criticisms, with some forcefulness? As κενός means empty or worse, as coenum or caenum means dirt or worse, as the announcement of a book on caenogenesis provoked the most violent astonishment ("heftigstes Staunen"), as the author, whose work

was discussed in the last volume of *Natural Science,* had dictionaries sent, if not hurled, at him, we think that he was right—for this is an age of compromise—in sticking to kainogenesis, and he seems to have Gegenbaur and other great authorities on his side. But to those who believe that kainogenesis is a term for an empty conception, the reading cenogenesis will doubtless seem preferable, for the German lexicon states that κενός means (1) leer, (2) vergebens, (3) eitel, (4) müssig, (5) ausgeleert. But, after all, the gist of the matter is rather that we should be sure that there is such a distinction as that between kainogenetic and palingenetic, before we become excited in regard to our spelling of it.

Flora of Sand Dunes.

THE flora of sand dunes has always been of great interest to botanists from the number of peculiar species which it offers, and also—especially more recently—from its remarkable oecological importance. The climatic and soil conditions under which it exists are so extreme in character, and vary so continually, that it offers a suitable field for the study of many problems dealing with the interaction of plants and their environment. Partly from this reason, and partly because of the absence of any complete study of dunes beside fresh water, Dr. H. C. Cowles of Chicago University has just published (*Bot. Gaz.* xxvii. 1899, Feb. to May, Fig. 26) an elaborate account of the general relationships of the dune vegetation of the shores of Lake Michigan. This paper is the first of a series on the subject, and treats of the geographical aspect. The extent of the whole area considered is great, but most attention is paid to the south-east coast of the lake, where the dune formation attains its maximum development—being largely due to the action of north-west winds.

In comparing those dunes to these familiar to observers in Europe the resemblances are much more conspicuous than the differences. It is remarkable how well many of the descriptions might be applied to the dunes around the British coast, if only the names of the species of plants were replaced by those of their European equivalents. Thus on the beach, where we should find *Cakile maritima,* Dr. Cowles records *C. americana.* On the loose dunes of both continents *Ammophila arundinacea* is the dominant and most important sand-binding grass. The plant associations in both cases include those of the xerophytic ridges, the intermediate swamps, and the mesophytic woods. In this country *Salix repens* fringes the travelling dune, in Michigan it is replaced by *S. glaucophylla* and *S. adenophylla*; here *Pinus sylvestris* and *Betula alba* are the dominant trees on the fixed dunes, there it is *Pinus banksiana, Betula papyrifera, Thuya occidentalis, Fraxinus americana,* etc. Many of the observations made by Dr. Cowles with

regard to the movement of dunes have their correlatives in this country. In this as in other ways the paper claims as much attention from European students as from those in America. The author suggests the problem offered by the presence of so many maritime and salt-loving species along the shores of a fresh-water lake, but reserves his explanation for a future paper, where he will particularly consider the oecological adaptations of the plants. The paper is profusely illustrated by process-blocks from photographs which, although they have undoubtedly suffered in reproduction, yet add greatly to the interest and value of the work, and aid in rendering it one of the most important oecological studies which has yet appeared in the United States.

Galway Natural History Museum.

We have from time to time given accounts of local museums, and our contemporary the *Irish Naturalist*, following our example, has in its June number a description of the Natural History Museum, Queen's College, Galway, by Prof. R. J. Anderson. From this interesting account we select two paragraphs:—

"Metropolitan museum authorities have sought to give a natural character to their collections, which one seeks for in vain amongst the average stuffed animals with their sleepless eyes and too cowering or too rigid pose. The example so well set has been followed here. One case represents a tug-of-war between an owl and a stoat, the rope is represented by a rat. Another shows the platypus at home with the avenues to his burrow by water and land; a third shows a peregrine and a slain rabbit; a fourth, a number of water birds with scenery; a fifth, the hornbill at home; a sixth, a fox interested in a woodcock; a seventh, an owl giving portions of a dead bird to its young; and eighth, a stoat with water birds, water, a dace, and a water-beetle; a ninth—a spider with a humming bird in his clutches."

"Proximity to the sea makes it possible to secure quite a number of living specimens. . . . I note on a window, as I write, a good many invertebrate types, living and well, sea-anemones and starfish, nereids and periwinkles, crabs and tunicates, crickets and spiders. In one tank are frogs and fresh-water mussels, in another tadpoles."

Botanical Biography.

We are glad to note the issue as a separate publication of the first supplement to Messrs. Britten & Boulger's Biographical Index of British

and Irish Botanists. It includes the botanists who died between January 1, 1893, and December 31, 1897, and also several who were omitted from the original Index, comprising together about 250 entries. There are a few well-known names, such as Babington of Cambridge, Huxley (whose claim as a botanist rests on a paper on gentians), Williamson, the expositor of the plants of the coal-measures, Bentley of the Pharmaceutical Society, his one-time associate author Trimen of Ceylon; but the great majority are not widely known, and many are to hand only as the result of painstaking research. By recording so many of these obscure, but often extremely useful workers, the authors of this Index have rendered a lasting service to Botany, and we shall hope for a regular recurrence of the supplement as time and botanists pass.

A New Found Trilobite from Newfoundland.

THE trilobite which Dr. G. F. Matthew has recently described in the *Bulletin Nat. Hist. Soc. New Brunswick* (vol. iv. No. 17, 1899) is of considerable size. The head shield is more than six inches wide, and the movable cheek with its greatly produced genal spine is about seven inches long. Its principal interest appears to consist in its supplying "a new link between the Cambrian of Europe and that of America." For certain Cambrian trilobites discovered in Sardinia, Bornemann founded the genus *Metadoxides,* characterised by a conical glabella as distinguished from the club-shaped glabella of the older genus *Paradoxides.* The glabella is conical in Dr. Matthew's new species from the Lower Cambrian beds of Newfoundland, and he describes it under the name *Metadoxides magnificus.* But he urges that it is a more primitive member of the genus than the Sardinian species, and, moreover, that *Paradoxides,* though older in name, is not older in nature than *Metadoxides.* He gives reasons for supposing that trilobites migrated from New Brunswick through Newfoundland to Southern Europe. To emphasise his views on the succession in time of various species, at the close of his article he proposes to divide the genus *Metadoxides* into three sub-genera, the first and eldest being *Catadoxides,* with the new *magnificus* for its exemplar. The late Henri Milne Edwards refused to accept the separation of *Olenus* from *Paradoxides* as a needless new-fangled addition to overburdened nomenclature. We can imagine, therefore, how charmed he would have been to be confronted not only with *Olenus* and *Protolenus,* and *Olenellus* and *Olenopsis,* but also with *Catadoxides, Metadoxides, Anadoxides,* the three sub-genera or infant progeny of *Metadoxides,* with the second child endearingly named after its parent.

Mexican and Central American Squirrels.

In the first volume of the *Proc. Washington Acad.* (pp. 15-106), Mr. E. W. Nelson attempts a revision of the species of squirrels inhabiting Mexico and Central America. In these days of "scrappy" papers, it is always refreshing to meet with anything of the monograph type; and a welcome should therefore be extended to this communication, even if we fail to accept all its conclusions.

The most generally interesting part of the paper deals with the degree of development of the fur of these rodents, according to the nature of the climate they inhabit. "The effect of climate," writes the author, "on the character of the pelage is so marked, that it is possible to tell with considerable certainty whether a species belongs to the tropics or to the higher mountains. Tropical species have thin pelage, short thin under-fur, and coarse, stiff, or almost bristly dorsal hairs; those of the Transition and Boreal zones have thick, soft pelage, with long dense under-fur. . . . Species of the hot coasts of Central America are characterised by peculiarly coarse, shining, bristly dorsal hairs. Seasonal differences in pelage are usually slight, since there is no area of heavy snow-fall or long-continued cold weather except in the Sierra Madre of Durango and Chihuahua. Individual variation, on the other hand, is often excessive, and renders some species extremely difficult to describe."

This, so far as it goes, is zoology in its highest and best sense. With regard to the descriptive portion of the paper, it must suffice to say that while the author finds it necessary to split up the genus into a number of groups, it is satisfactory that these are regarded in the light of sub-genera rather than distinct genera.

Spinning at Dawn.

Dr. Emil A. Goeldi, the enthusiastic director of the museum in Pará, tells an interesting story of an early rising spider—*Epeiroides bahiensis* Keyserling by name. The spinner was common in his garden, but the web defied discovery until Goeldi's son Walther, a boy of seven, sat up to detect the trick. The fact is that the spider makes its web in the early hours, and rolls it up and decamps after the sun rises. Penelope-like it destroys its web daily, but not without result to man as well as to itself, for it catches the minute winged males of the destructive Coccidae, of *Dorthesia americana* in particular. After retiring under the shade of a leaf the spider investigates the insects in its rolled up net, and spends the hot hours in digesting them. Its behaviour reminded Goeldi of a southern bird-catcher hastily gathering his roccolo together as the dawn breaks, but with this difference that

the spider "does not stop to pull out the captives, wring their necks, and throw them into a bag. It gathers up its net and postpones the work of revision until it gets home." This interesting paper will be found in *Zoologisches Jahrbuch*, xii. (1899), pp. 161-169, 1 pl. and 1 fig.

E pur si muove!

We could not find a finer instance of the progress of science—which it is part of the function of our journal to record—than Dr. (now Sir) J. Burdon Sanderson's Croonian Lecture, delivered to the Royal Society of London on March 16, "On the relation of motion in animals and plants to the electrical phenomena which are associated with it."

The progress to which we refer might be best indicated by a summary of the actual results and suggestive hints to which the lecture gives expression, but it seems more picturesque and not less important to cite the first two paragraphs, for they indicate as it were graphically the strides of modern physiology to which the baronet's genius has given so much force.

"In a Croonian Lecture which I delivered to the Royal Society in 1867—more than thirty years ago—I exhibited a number of diagrams of graphic records in evidence of the mechanical relations which I then sought to establish between the movements of the heart and those of respiration in the higher animals.

"I have to-day to bring before you results which have also been obtained by a graphic method, which however differs from the other in that the records are written by light, and not by pen on paper; that the time taken in recording is measured in thousandths of seconds, not tenths; and finally, that the events recorded are not the movements of the chest or heart, but the electrical changes which, as will be shown, are found to associate themselves with all manifestations of functional activity in living organisms, whenever these take place under conditions which admit of their being investigated."

A Complementary Male.

Many years ago Darwin discovered a little creature living on the barnacle, *Scalpellum vulgare*, which he at first regarded as a parasite and afterwards as a "complementary male." In other cases, as is well known, he found a similar dimorphism,—minute complementary males fixed to the hermaphrodite barnacles, and in some rare species to females. Since Darwin's work there has been little if any re-investigation of the complementary male of *Scalpellum vulgare*, but it has recently

found a careful student in Mr. A. Gruvel (*Arch. Biol.* xvi. 1899, pp. 27-47, 1 pl.). In Hoek's *Challenger Report* there is some account of the complementary male of *Sc. regium*, which is said to have two ganglia, a functionless stomach, and cement glands, but not much else. In the species studied by Gruvel the male is also very simple. It has two ganglia and an eye, but no digestive canal nor specialised vascular and respiratory apparatus. It is little more than an independent testicle endowed with a minimum of individuality.

Mr. Gruvel finds it difficult to admit that similar eggs fertilised by spermatozoa of the same origin produce larvae destined to give rise, some to hermaphrodites and others to these pigmy males. And so he has thought out a theory which may render the affair less mysterious, though we are not at all confident that it does. Cirripeds are usually protandrous, *i.e.* the spermatozoa ripen before the ova. The sperms are shed first, and accumulate in the interpallial space. By and by the ova pass into the ovigerous sac, and are there fertilised; as they develop, the gaps in the sac are closed, and the whole is detached from the genital atrium to be fixed to the ovigerous frenum. Thereafter there emerge belated ova which have a poor chance of being fertilised by the spermatozoa of the hermaphrodite. And Gruvel's theory is that these are fertilised by the spermatozoa of the complementary male, which are usually longer of developing than those of its bearer, and that from these ova thus fertilised complementary males are produced.

Is Fertility Inherited?

In the sixth of his valuable memoirs entitled "Mathematical Contributions to the Theory of Evolution," Prof. Karl Pearson, with the assistance of Miss Alice Lee and Mr. Leslie Bramley-Moore, brings forward evidence to show that fertility is inherited in man, and fecundity in the horse, "and therefore probably that both these characters are inherited in all types of life"—in all likelihood according to the Galtonian rule. We have only seen the abstract in the *Proceedings of the Royal Society* (lxiv. 1899, pp. 163-167), but that is enough to show the interest and importance of this inquiry, especially in connection with "reproductive or genetic selection"—a term (which seems to us unfortunate) used to describe "the selection of predominant types owing to the different grades of reproductivity being inherited, and without the influence of a differential death-rate."

Mr. Pearson points out that the problem of whether fertility is or is not inherited is one of very far-reaching consequences. "The inheritance of fertility and the correlation of fertility with other characters are principles momentous in their results for our conceptions of evolution; they mark a continual tendency in a race to

progress in a definite direction, unless equilibrium be maintained by any other equipollent factors, exhibited in the form of a differential death-rate on the most fertile."

He seeks to force biologists to face a dilemma. If the above principles are accepted, then the biologist "must look upon all races as tending to progress in definite directions—not necessarily one, but possibly several different directions, according to the characters with which fertility may be correlated—the moment natural selection is suspended; the organism carries in itself, in virtue of the laws of inheritance and the correlation of its characters, a tendency to progressive change." If, on the other hand, the biologist does not accept the principles, then he must be prepared to meet the weight of evidence in the memoir. But is it not fair to remark that this evidence relates to two highly artificial cases—man and the race-horse?

Living Fossils.

WHETHER Mr. J. E. S. Moore is correct or not in his interesting hypothesis that Lake Tanganyika represents an old Jurassic sea, and that many of the molluscs in it are long-lived relicts of Jurassic fauna, he must get credit for his careful and enthusiastic endeavours to make good his case. We believe that there are some who are in no way convinced, and it was with interest therefore that we read Mr. Moore's continuation of his previous studies on the molluscs of this great lake (*Quart. Journ. Micr. Sci.* xlii. 1899, pp. 155-201, 8 pls.), in which he deals with forms called *Tanganyika rufofilosa, Spekia zonata, Nasopsis nassa,* and *Bythoceras howesii,* which he found on the picturesque shores, or dredged from the deep waters.

His conclusion, on which it would be unfair to throw doubt without detailed criticism, is that all the evidence which has been collected concerning the nature of the halolimnic Gastropods invariably points to the vast antiquity of these forms. "First we have the wide dissimilarity of their empty shells from those of any living types; next their rigid isolation to a solitary great lake, which, judged from whatever standard we may choose to adopt, is unquestionably of an enormous age. Next we have the wonderful similarity of the halolimnic shells now living in Tanganyika, to those which have been left fossilised at the bottom of the old Jurassic seas; and lastly, there are the morphological characters of the halolimnic animals themselves, whereby they become mentally depicted like nothing so much as the incompletely developed embryos of numerous living oceanic types."

ORIGINAL COMMUNICATIONS.

Notes on the Habits of the Northern Fur Seal.

By G. E. H. Barrett-Hamilton.

Introduction.

There is probably no species of wild mammal to whose life-history so much attention has been paid as the Northern Fur Seal (*Otaria ursina*). For about a century and a half a source of wealth to large and powerful companies, it was after the first discovery of its breeding haunts by the ill-fated Vitus Bering in 1742, the object of a slaughter as indiscriminate as it was inimical to the permanent interests of those who took part in it. In later years, however, when a diminished herd plainly foreshadowed the fatal effect of this foolish destruction of valuable animals, every effort has been made to preserve the seals, and they have been for some time the objects of the most careful study on the part of the governments who own their breeding haunts, a study which culminated in the appointment of the International Commissions of 1891-93 and 1896-97.

Volumes upon volumes have been devoted to the Northern Fur Seal; of these, very many are blue-books, or government publications, a large portion of which are of too patriotic a nature to be safely relied upon by scientific men. Some other accounts of the seals, which cannot be included in the above category, have been tinged with a depth of poetical imagination obviously intended for popular rather than scientific reading, so that the Commission of 1896-97 found much to correct or supplement in our knowledge of even the most simple features of the life-history of the animal. Bearing this in mind, I think I need no excuse for putting together a brief account of the observations which I made during my visits to the rookeries. In doing so I shall entirely exclude all matter relating to the commercial or diplomatic questions at issue, and I hope my notes may be taken as a perfectly unbiassed account of what came under my own notice.

Before I go further, it may be well to state that I assume that all naturalists are acquainted with the general facts of the life-history of the

Fur Seal, so graphically described by Mr. H. W. Elliott: how the herds which spend the winter months in the warmer waters of the Pacific south of their island homes, move gradually northwards in the early part of the year, and in spring, land on the rookery shores, the females to give birth to their young, the old males to commence a jealous watch over their hardly-won harems, which they only forsake when hunger and fatigue or the valour of a rival forces them to leave their posts; how the young males, unable to face their seniors and win for themselves places on the coveted rookery beach, while away the summer in sleep and frolic on their own hauling-grounds, whence the sealers take their toll of skins; how the seals remain in the neighbourhood of the rookeries until the cold gales of autumn warn them to again depart southward. Such, in broad outline, is the natural history of the Fur Seal, and with such general matters of common knowledge I have here nothing to do. It will be my business rather to attract attention to certain of the less known features of what I may call the social life of the animal.

I assume also a knowledge of such sealing terms as bull, cow, bachelor, pup, harem, rookery, and hauling-ground. Any further technical terms which it may be found necessary to use will be explained as the occasion arises.

It must be clearly remembered, however, that my visit to the rookeries was paid at a time when the numbers of the seals had admittedly decreased since the date of the descriptions of some of the older authorities, as, for instance, those of Mr. H. W. Elliott. Hence, if what I saw does not always quite closely correspond with the observations of older naturalists, it does not necessarily follow that one or the other of us is in the wrong. It may be that both they and I are right, and that the differences which it is our duty to record actually existed and are due to the prevalence of different conditions on the rookeries at different times, consequent on their disturbance by man.

My Experience.

My personal experience of the Northern Fur Seal was gained in the two breeding-seasons of 1896 and 1897, during which I actually lived in turn on every island where there is any important rookery at the present time. On one island or another I had the seals under my observation almost throughout the duration of their summer stay on land. My movements were as follows:—In 1896 I gained my first introduction to the seals at the small rookery on Robben Island (in the Okhotsk Sea), which I examined on July 11. In the same year I spent July 19 to August 10 on Bering's Island, and August 11 to 25 on Copper Island, on the western side of the Bering's Sea. I spent September 1 to October 4 on St. Paul Island (including two days at sea among the pelagic sealers in the United States Revenue

cutter "Rush"), and October 4 to 22 on St. George Island, thus missing only the earlier part of the season of 1896. In order to complete my knowledge, and to be able to observe the seals in the earlier part of the breeding-season, I reached Bering's Island in 1897, on June 19, and remained there until August 2, when I sailed for Copper Island, and landed there on the following day. On August 19 I left Copper Island in an unsuccessful search for seal rookeries on the Kamchatkan coast, and did not again return to the seal islands. During my stay on the islands I personally examined and walked over the whole extent of all the rookeries, with the exception only of one or two of the lesser ones on the Commander Islands, which I had to be content to observe through my binoculars, either because they are inaccessible from the land side, or because I had not permission to approach them more nearly.

Enumeration of Seal Islands.

The islands whither the seals resort for breeding purposes are now all well known, and it is unlikely that the most diligent search can add to their number. They are the Commander and Pribilof Islands in Bering's Sea, certain of the more Northern Kuril Islands,[1] Robben Island (in the Sea of Okhotsk), and possibly one or two other small rocks[2] and islets in the same sea. It can hardly be doubted that the presence of the seals on these islands, apparently scattered at random throughout the North Pacific, and their absence from many others equally suitable for their purpose, such as the Aleutians, depends entirely on the former presence or absence thereon of man. The Commanders and Pribilofs are the only large uninhabited islands in the North Pacific, and there are no rookeries on the Aleutian Islands, which, although affording very suitable conditions, are inhabited throughout their extent.

What Guides the Seals in their Choice of an Island or Rookery.

A glance at Sir John Murray's map (*Geographical Journal*, August 1898) to illustrate the annual range of the surface temperature of the ocean will show that the question of temperature has had no very great influence on the choice of the seals of islands on which to bring up their young. Whereas the Pribilof and Commander Islands lie in regions where the surface temperature is cold, and has an annual variation of only 20° F., the corresponding figure for the Northern Kuril Islands is 30°, while the little rookery at Robben Island lies close to the border line of regions where the annual variation amounts to 35° and 40° F. respectively. Again, whereas in the event of a backward

[1] Shnednoi, Raikoke, and Mushir.
[2] St. Iona and the Shantai Islands.

spring the seals must await the dispersal of the ice before they can land on the colder shores of the Pribilofs and Robben Island, the ice-free Commanders are always ready to afford them a safe resting-place. It is obvious then that what they chiefly want are uninhabitable islands which are free from ice and snow by the time at which they wish to land. On such islands breeding seals are not at all particular as to the nature of the ground they lie upon, provided only that it is not a sandy beach. Such a beach seems to cause them some annoyance, probably because the particles of sand (especially in wet or windy weather) stick in their fur and irritate their eyes. The non-breeding seals or bachelors have, however, no such aversion to sandy beaches, and are frequently to be found hauling up on such, especially on the great sandy bays of St. Paul Island. In the latter case, however, it may be that they haul up on sand not because they like it, but because all other suitable areas are occupied by breeding seals, and hence forbidden ground to the bachelors.

The only rookery where I saw breeding seals hauling up on sand is that of Robben Island, and here the shingle which composes the beach becomes in some places gradually finer, so that it is actually of the consistency of coarse sand. In addition to Robben Island there are one or two sandy spots frequented by breeding seals at St. Paul's, but these are small and chiefly brought into prominence by the ravages of the parasitic worm (*Uncinaria*) among the pups born on these flat sandy surfaces.

Elsewhere the rookeries and their situations are as varied as they could well be. Thus on St. Paul Island the seals, finding flat areas gently sloping up from the sea, have overrun whole acres of the island, even ascending the sides of hills, which lie at a distance of several hundred yards from the beach, and reducing the whole area occupied by the rookeries to a bare expanse of stone and clay, long since worn quite clear of grass or vegetation by their constant passage over it. St. George Island is more mountainous, and here the seals are forced to occupy more rocky ground, only advancing up the cliff-sides where the nature of the ground permits their easy ascent. On Bering's Island one rookery is on a great reef, while the other is on a narrow beach at the foot of a low but unscaleable cliff. Lastly, we have the opposite extreme in mountainous Copper Island, where the high sheer precipices leave the seals no choice but to occupy the narrow beaches, small inaccessible bays, and projecting reefs, which alone intervene between the island and the sea. Yet even here, when opportunity offers, they climb up the gulleys formed by streams which have here and there cut a channel for themselves through the cliffs on their way to the sea, and, as at Palata, wear out for themselves a bare parade ground above the level of the shore. Naturally the best sites for rookeries are sheltered bays where projecting reefs shield the young pups from the violence of a heavy surf and form pools where they can

play and learn to swim in safety. Such bays are to be found on Copper Island at Gavarushkaya and Sikatchinskaya, while parts of the great northern rookery of Bering's Island are fairly well protected from storms. Thus on shore all sorts of ground seem suited to their wants, except, as already noticed, flat sandy areas, and beaches in the too close proximity of overhanging cliffs. Here landslips have been known to occur, burying and killing a number of the cows, as at Palata in Copper Island; while at Orilli Kamen, another Copper Island rookery, I found the skeletons of three unfortunates (one of which at least was a bull) under a great boulder which had fallen down from the cliff above the rookery and crushed them. But perhaps their most favourite haunts are cliffs where the slope is not very steep and large boulders lie plentifully strewn on the face. Here they ascend often to a height of a hundred feet or more, easily traversing places where a man could hardly climb. Such cliffs are very numerous at St. Paul Island, and here seals may be found asleep in all sorts of strange retreats on the cliff-sides, whence, if unexpectedly disturbed, they will often jump blindly down a steep incline, facing a fall that would kill a man. The little pups, too, are very fond of lying asleep with their heads, or sometimes their whole bodies in holes, under rocks. When disturbed they rush in hot haste, "baaing" lustily, in any direction in which at the time their nose happens to be turned, not looking in the least to see whither their precipitate flight will lead them.

Robben Island—Comparison of Mr. Elliott's Observations.

My first acquaintance with the Fur Seal was gained at Robben Island, and a mere glance at the little rookery there was sufficient to show that neither is the animal, as a whole, deserving of the reputation for intelligence with which Mr. W. H. Elliott has clothed it, nor is the cow the sweet-tempered, dove-like creature which the same writer has described. Not only were the bulls exceedingly active and constantly engaged in rushing blindly hither and thither, utterly regardless as to whether they trampled the cows or pups under their flippers, but the cows, although they sat huddled closely together as if in a state of affectionate good-fellowship, were constantly snapping at each other in a bad-tempered manner, and savagely resented the approach of all pups except their own. A dead pup which I picked up at some little distance from the rookery showed, on examination, that it had received a bite, probably from a cow, on the head, where the punctures made by two canine teeth were plainly visible in the thick skin. The greater part of the head was in a rotten and putrid condition as if a fatal erysipelas had set in as a result of the bite.

Variability of Seals.

A point which at once strikes a visitor to a seal rookery is the great variability in the colour and size of the animals. There are indeed limits to such variation, but within these limits the Fur Seal of almost all ages cannot but be regarded as a most variable species. The same is true also of skulls of the animal, and differences can easily be found in specimens from the same rookery such as would, if they were constant and each confined to specimens from particular localities, undoubtedly warrant their division into several distinct species.

Observations on the Rookery.

It was one of my objects to observe the first landing of the seals on the islands, in order, if possible, to test for myself the trustworthiness of Mr. Elliott's wonderfully graphic description of their habits at this season. Accordingly I endeavoured to reach the islands at as early a date as possible in 1897, and actually arrived at the North rookery of Bering's Island on June 20, or very soon after the appearance of the first seals. I then visited the South rookery of the same island, and pitched my tent there on June 23, with the intention of remaining for at least a week. Finding, however, that the state of things at the South rookery was not exactly what I needed for the study of the seals, I left it on June 26, proceeding by dog-sledge to the North rookery, where I arrived on June 29. Here I remained four days, during which almost my whole time was spent in watching the seals, chiefly at the part of the rookery known as Kishotchnaya. I was informed that there had been present on June 16, 13 bulls, 110 cows, 37 pups, and 5 bachelors. On June 20 I found the 13 bulls thus disposed :—

5 with a mass of at least 175 cows and a number of newly-born pups.
1 with 6 cows and 3 pups.
2 with 2 cows each.
1 with 1 cow.
1 lying asleep near the bachelors.
2 alone to the south of the main patch of cows.
1 alone in another position near the main patch of cows.

At this date I take it that the rookery showed the condition in miniature which a well-regulated rookery, of whatever size, should show at the height of the season—that is to say, there were a certain number of strong bulls which had appropriated to themselves large harems, in this case averaging over thirty-five females each : there were

other bulls who had to be content with harems containing from one to six females each, while there were yet again other bulls which were as yet unable to get among the breeding females at all, and which represented the "idle" or "reserve" bulls of the Pribilof Islands.

Several points struck me in connection with the habits and disposition of the bulls during the earlier parts of the season:—There were at the North rookery no bulls anxiously awaiting the arrival of the cows on the shore-line. The best stations were evidently not on the shore-line, but at the places where the patches of first-arriving cows were massed together, and it was to these patches and not to the sea that the attention of the still unoccupied bulls was directed. Many of the bulls, both of those which possessed harems and those which did not, were asleep, and were not displaying that almost ceaseless activity which a perusal of the writings of Mr. H. W. Elliott would lead one to expect.

The cows were not received by the bulls at the shore-line, but seemed to come in unnoticed and quietly joined one or other of the patches of their sisters who had already arrived. Sometimes a cow was delayed in her progress up the beach by the unwelcome attentions of one or other of the wandering half-bulls which had not yet gained a harem, but such delay was seldom of long duration, as the cows were very persistent in their movements and resented as angrily as they dared all attempts of the half-bulls to stop them.

The rookery in its first beginnings did not consist of a large area of loosely scattered bulls and cows, but of the above described densely crowded, although small, patches. It is thus interesting to note the passage by a large rookery early in the season, although in the reverse order, through the stages exhibited by one which is in the course of being exterminated. The former starts as a number of detached and crowded patches, which in the end coalesce and fuse to form one rookery; the latter musters in the early part of the season in exactly the same manner, but the patches may never grow large enough to coalesce and fuse. In spite of the crowded condition of these patches, the cows were, as at Robben Island, constantly quarrelling with and snapping at each other. The bachelors, no doubt owing to the great proportion of old and unoccupied bulls present, were hauled up in one lot by themselves, and amongst them were several of the large half-bulls, which later in the season were acting as masters of harems on the breeding-grounds.

The bachelors appeared to be ready to stampede had they been approached too closely, but the bulls and cows could not, I think, have been moved except by force. The bulls roared at us and were very threatening, but would not leave their cows to attack us. All the bulls appeared to be in good health, but in a variable state of fatness.

As the season advanced and the area of ground occupied by the rookery increased, it was obvious that the small harems seen by me on June 20 were merely the nuclei of larger gatherings, which gradually increased and swelled so much as to coalesce and form the rookery as seen in its completed aspect. Thus those bulls which were at first obliged to sit outside the harems were for the most part absorbed in the breeding-grounds, and, as at the Reef section of the rookery, the bachelors found no difficulty whatever in wandering among the cows.

By the 29th June the females had so increased in numbers as to be quite out of the control of the bulls, and they were then able to make their way to or from the sea with little or no interruption. Many of them lay in loosely scattered patches with no bull to attend on them.

Yet the strange thing was that, although in several cases the harems of individual bulls grew to such unwieldy proportions that the bulls were powerless to prevent the cows from leaving them or from joining other bulls, there were all the time other bulls which, either from the position which they had selected or from other reasons, were never able to secure a harem. Their desire was evidently to occupy some particular position already commanded by a stronger bull. This being impossible, they sat or slept out of reach of their enemy, and made no attempt, as a rule, to collect a harem for themselves.

Occasionally, however, one of these solitary bulls would become infuriated, and, charging down upon the harems, would seize a female and run away with her. The female, however, thus captured invariably, as far as I could see, returned to her old place at the first opportunity.

Although not possessing harems, these bulls were by no means idle, for they often had a single cow with them, which no doubt had been dissatisfied with her treatment at the hands of the master of her own particular harem, and had sought another lord. The visits of such cows to these outlying bulls appeared to be of a merely temporary nature, and I think they returned to their own harem when satisfied by the accomplishment of their object in leaving it.

Some of the harems which I kept under close observation for several days will illustrate these points.

There were at the south end of Kishotchnaya during the early part of the season four bulls by themselves; one of these had on the 29th June about sixty-three females and another twenty; while not far from them sat three younger bulls, one alone and the other two with three females.

The following table shows the increase in the two larger harems from day to day:—

	June 29.		June 30.			July 1.	July 2.	July 3.
	P.M. 2.30	P.M. 6.15	A.M. 10.40	P.M. 3.15	P.M. 6.30	A.M. 10.35	A.M. 11.55	P.M. 12.10
Harem I., number of cows .	63	64	56	63	64	90	69	89
Harem II., ,, ,, .	20	24	24	24	34	42	52	72
Total number of cows in the two harems . . .	83	88	80	87	98	132	121	161

Now, although these harems thus increased from day to day, so that in four days the number of cows was about doubled, and the cows, being in the proportion of (on the 3rd July) eighty to the bull, were completely out of control and free to move about as they wished, yet during all that time there were bulls hovering round the outskirts of the harems, some of which were masters of no cows, and none of which had succeeded in collecting a greater number than three each. Nothing could better illustrate the fact that it is the cows, and not the bulls, which have the real control of the harem-system. Over these 161 cows the bulls, in spite of all their bluster, had the flimsiest of nominal dominion, and the cows were always able to, and frequently did, leave their harems to dally with cowless bulls on the outside. Yet, whether their number was 80 or 160, as long as they chose to sit massed together on the ground which had been appropriated by the two stronger bulls, no weaker rivals could approach to within a distance of 10 yards from them. The master of the harem had no control over its occupants, but he was absolute lord of the ground on which they sat.

An almost better illustration of this was to be seen at the South rookery, where, later in the season, there were often 200 cows on shore with two bulls. Yet (as on the 26th July, when there were 287 cows on the beach) the division of the cows into harems was a very unequal one, the smaller bull being only able to keep a very few cows, while the larger one claimed the greater part of the rookery. But the cows could pass over to the smaller bull's ground as often as they liked, and he probably was father to a great many more of the pups born in 1898 than those of the half-dozen cows over whom he claimed control.

At the same rookery on the 28th July, when there were over 190 cows on shore, the whole of this number was greedily claimed by the larger bull, while the smaller bull was forced to sit apart outside the patch of massed pups which lay just outside the rookery. True he sometimes threatened to make descents on his rival's harem, but he had no cows that he could really call his own until they themselves took the initiative and went out to join him.

Thus the inequality of the two harems at the North rookery kept increasing until there came a time when the newly-arriving cows began

to lie in scattered groups outside the main mass, and thus permitted the weaker bulls to form new harems out of the reach of the two strong old bulls.

The following table shows the number of bulls and cows on the western portion of Kishotchnaya outside of the two larger harems:—

	June 29		June 30.			July 1.	July 2.	July 3.
	P.M. 2.30	P.M. 6.15	A.M. 10.40	P.M. 3.15	P.M. 6.30	A.M. 10.35	A.M. 11.55	P.M. 12.10
Total number of cows in the two larger harems (as before)	83	88	80	87	98	132	121	161
Number of other bulls in this section	4	4	...	...	3	4	3	?
Number of outlying cows .	1	5	...	...	1	3	16	18

A fact which came under my observation in connection with the bulls and half-bulls was the fact that several of those which had a regular station on the rookery occasionally absented themselves from it. Thus, one bull at Kishotchnaya was absent from his place during the earlier part of the 1st July. In the evening I was fortunate enough to see him return. At 1.20 P.M. on the 2nd July this same bull—a grey one, and therefore probably of no great age—left his place in the rookery, and passed out to a position less than 100 yards away on the reef. Here he slept until 3.20 P.M., when he awoke, deliberately returned to his place on the rookery, and scattered the other bulls who attempted to face him.

In 1896, too, I had observed the same phenomena. Thus on July 23, whilst some of the isolated patches of seals at the section of the North rookery known as the Reef were under my observation, I saw a very black-looking bull coming across the sands towards the rookery from the west, and apparently from the sea. When this bull approached the rookery more closely several of the others began to make demonstrations against him, rushing out for some distance from their harems to meet him. At first the intruder seemed to be frightened by the show of hostility with which his arrival was greeted, and slackening his pace, sat down as if to rest and think things over before approaching within fighting distance. Thus I got a snap-shot of him. He was, however, only taking his own time about his own business, and presently he went deliberately into what he evidently considered his own place, the other bulls retiring before him. From the first his action was deliberate, and he made for one particular part of the rookery as if he had known it all his life. These roving habits on the part of a full-grown rookery bull were so unlike anything of which I had read previously, that they gave me a good deal of trouble before an explanation was forthcoming. At first I was inclined to attribute them to possible disturbances of the rookery during the

course of driving the seals, by which this bull had been driven into the sea (as I have seen many others during the course of a drive on the very same ground), and had not returned for some hours. But later in the season on the Pribilofs, on the little undisturbed rookery of Ardiguen, there was under the observation of our whole party a bull who, after having held his own place valiantly before all comers throughout the season, at length retired to the sea for rest and food. But to our surprise we saw him returning fat and sleek after a few days' absence, and during the rest of my stay on the island he continued his assiduous attentions to his now attenuated harem, varied only by occasional visits to the sea. It appears, then, that there is a good deal more latitude and deviation from their habits on the part of bulls than one would have supposed from reading the earlier accounts, and there can be no question that some of the bulls which frequent the rookeries of the Commander Islands come and go to and from the sea and their harems even at the height of the breeding-season, but that others (as noticed at the Pribilof Islands) only assume these wandering habits at or near the close of that period. I never saw a bull that I was certain was a really old one behave in this irregular fashion, and the old yellow-looking bulls of the central massed portions of the rookery never left their places even for an instant, so far as I could see. It may be, therefore, that the irregularity occurs only among the younger bulls, and is due to the system of management of the rookeries, whereby the number of spare bulls has been diminished, so that young animals have no difficulty in gaining harems for themselves at an age when their strength would certainly have been insufficient to have enabled them to do so in a state of nature. At all events, such wandering habits are normally those of the larger bachelors and half-bulls, who, when unable to gain access to the harems, pass a restless life on their outskirts, varied with occasional—in the case of the younger animals frequent—visits to the sea. To these habits the two bulls of the little South rookery of Bering's Island reverted at the end of July (1897), first becoming restless and moving about a good deal before they left the rookery for good.

On the 13th July, on which date the North rookery was visited by Dr. Stejneger, Professor D'Arcy W. Thompson, and others, it was found that there had been a marked increase in the number of the seals, both in the case of the females and, what struck me more, in that of the bulls. The western section, which had never contained more than six bulls and 179 cows on any previous occasion on which we had visited it, now included a number of cows which was variously estimated at from 500 to 700 individuals. With these, from seven to ten bulls were noticed by the various observers. The area occupied by the seals had greatly increased, and the harems which had been previously under observation were now indistinguishable ; the places of the two bulls were, however, occupied, if not by the same animals, by similar or identical

ones. All around their stations were new harems, which had been formed by late arriving cows, attended on by bulls which had previously possessed only a few cows each.

At the Reef or eastern section a very similar state of things prevailed, and on the 13th July not only was the number of cows on shore vastly greater than on any previous occasion in that year, but the bulls had also increased in numbers in a manner for which, I confess, I was totally unprepared.

A point which struck me very forcibly in regard to the new bulls on this day was that they were, in my opinion, all young bulls, that is, they were blacker or greyer, as well as smaller, than the bulls which I saw during my earlier visits to the rookery, in the centre of the thickest masses of females.

The new bulls did not show the yellowish colour of the older bulls; they did not accompany the old bulls to the rookery early in the season when they arrived to await the coming of the cows; and they would not at that time have dared to approach within many yards of these old bulls. It was evident, in fact, that, like the cows and bachelors, these young bulls continue to arrive at the rookery until the height of the season, and that they do not accompany the older bulls, which arrive before and await the arrival of the cows.

I am unable to state the time at which the old bulls left the North rookery of Bering's Island, for in 1896 I was not there early enough to recognise them individually, and it was unfortunate that in 1897 neither Dr. Stejneger nor I were able to visit the North rookery after the 16th July, on which date I could recognise many of the bulls which I had seen on the rookery ground earlier in the season. At what time they took their departure it is impossible to say, but it seems reasonable to suppose that they did so at the same time as did the bulls of Copper Island, that is to say, at about the first week of August.

Behaviour of the Bulls.

The following notes will give some idea of the nature of the tasks which the bulls have to perform:—

At Kishotchnaya, in the harems which I kept specially under observation, a large half-bull was observed *in coitu* at 3 P.M. on the 29th June. Afterwards the cow and bull did not separate, but continued to sit near each other, and at 3.55 P.M. the act of copulation was repeated, on this occasion in from 6 to 8 inches of water. I then left the rookery, and returned at 6.15 P.M., at which time I found (apparently) the same animals for the third time *in coitu*, on this occasion in water in which both could swim; the operation took place largely when the animals were floating side by side in the water.

At about the same time (viz. 6.40 to 6.55 P.M.) another half-bull and cow were observed *in coitu* in a depth of from 2 to 3 feet of

water. On its termination the animals swam away in different directions.

On the 30th June the bull whose harem is numbered II. in the table on p. 25, was observed *in coitu* twice during the space of about one hour, the first time at about 6.34 P.M. He was afterwards active until about 7.33 P.M., when he again performed the act.

On the 1st July the same bull was observed *in coitu* twice during a period of six hours, that is to say, at 12.9 P.M. to 12.14 P.M., and again at 12.33 P.M. until 12.41½ P.M.

On the 2nd July he was observed *in coitu* four times during a period of four hours, viz. at 12.35 P.M. until 12.37½ P.M., at 1.30 P.M. until 1.35½ P.M., at 2.5 P.M. until 2.12 P.M. (in the latter case apparently futilely), and at 3.31 P.M. to 3.39½ P.M.

During a period of thirteen hours, in which on various occasions the two bulls were under observation, each was observed *in coitu* eight times.

If each bull kept up the same rate during a whole month of twenty-eight days, it is obvious that he could accommodate a harem of over 200 cows. The rate is, however, as shown by the above notes, not constant, and it happened that the periods of greatest activity of the two animals did not always coincide. This I put down to the varied times at which the cows came into heat, and from the notes which I was able to make it seems nearly certain that the cows are covered more than once each. The action of bull 1 during six hours, in which he was observed *in coitu* no less than six times, led me to believe that, in the case of several at least of the acts which I then observed, it was the same cow which was covered; but of this I cannot be certain, it being extremely difficult to keep any one cow under observation in a crowded harem.

On the South rookery of Bering's Island the two bulls are known to have been present from about the 5th July to the 1st August, a period of only about twenty-six days. Their departure at about the latter date may be assumed to have been due to either of two facts—viz. either there were then no females requiring their services, or else their power of accommodating the females was finished for the season. That the latter was the true reason seems almost certain, from the fact that there was a newly-born pup with its mother—probably a three-year-old cow with her first pup—on the rookery beach when I visited it on the 2nd August, and also from the fact that it is in the last week in July or the first week in August that the large old bulls of the Copper Island rookeries leave their harems and retire to the beaches north and south of the breeding-grounds and elsewhere.

We know that these two bulls at the South rookery had between them a lot of at least 530 cows, or 265 cows each. If each of these cows were covered only once during the twenty-six days, it would be necessary for each bull to satisfy about ten cows every twenty-four

hours throughout his season, and a very much greater number if any large proportion of the cows received a second service.

On this rookery there appears to have been only one bachelor large enough to assist the bulls, but he was not larger than a big cow, and does not seem to have exerted himself much: only on one occasion was the presence of three bulls (the third being probably the large bachelor) reported by the natives.

These two South rookery bulls were neither of them apparently very old: but one of them was a pretty large dark bull, with a light wig; the other, a smaller bull, was, as has already been stated, only permitted by his rival to remain at or near the edge of the rookery.

On the 24th July both these bulls appeared to be active, and each was observed *in coitu* at 3 P.M.

On the 25th July the smaller bull was noted to be looking thin, and was seen *in coitu* at 11.30 A.M. He seemed to spend most of his time in sleep, whereas the larger bull was more active, and constantly examined his harem as if to find a cow in heat.

On the 28th July, at 3 A.M., Mr. Volokitin (the Russian in charge of the rookery) noticed only one bull on the rookery.

By the 29th July the two bulls had begun to go into the water and to follow the females to the outlying rocks on the reef, and on the 30th, when I examined the rookery at 8.30 A.M., there were no adult seals on shore, and no bulls to be seen anywhere. Mr. Volokitin told me that one bull was on the beach on the 1st August, but there were none to be seen when I visited it on the 2nd August.

If the bulls were vigorous, the bachelors, down to the smallest of them, were equally so. In the earlier part of the season no bachelors were observed at the South rookery, but at the North rookery, as I have already said, I found them, when I first arrived there, lying in a pod by themselves apart from the breeding seals. As the season approached its height, and the number of cows so increased and spread over the ground as to render the task of the bulls who tried to restrain their movements a hopeless one, the bachelors began to mix amongst the females and to wander about among them much as they pleased. It was at this time that I was able to satisfy myself of the correctness of the observations, often described, of those who have seen the young bachelors covering the cows.

My attention was first drawn to this at 4.35 P.M. on the 30th June, by hearing the strange voice of a bachelor, neither quite like a cow nor quite like a bull, at the Reef section of the North rookery. I found that this proceeded from a small bachelor who was trying to cover a female, obviously in heat. Another and smaller bachelor also tried to cover the female, and then a bigger one coming by drove the small one away, and amused himself with the female until 5.3 P.M. She then escaped from him, being evidently satisfied, but he pursued her and

tried to prevent her leaving, in exactly the same manner as a mature bull would have done. These proceedings went on in water, in which both animals were practically afloat, and occurred at the edge of the patch of seals lying nearest to the land, and not far from the large bulls. The female was obviously in heat, since she allowed the bachelors to play with her. Several other small bachelors were constantly loitering about while the larger one was *in coitu.*

On the same day I saw another quite small bachelor trying to mount a female at another part of the same rookery, but she seemed to object, and a bull eventually drove him off. Later on I saw the bachelor in the shallow water annoying other females.

Such occurrences I afterwards saw frequently, the bachelors being in some cases actually smaller than the cow they attempted to cover, and only recognisable by their voice and for other reasons. In all such cases the bachelors behaved exactly as would have a large bull under the same circumstances, trying to keep the cows close to them in order to be able to cover them again. In some cases I saw cows which were certainly in heat escape from bachelors and pass right under the bull's nose without being covered, the bull's attention being too much taken up with other cows to notice them.

The same thing went on also at Kishotchnaya, where I first noticed it also on the 30th June. On the 2nd July I watched the harems of the two large bulls at Kishotchnaya (already alluded to) continuously from 11.55 A.M. to 4.8 P.M., and during this time the bull numbered I. was observed *in coitu* twice and the bull numbered II. four times. Yet during a good part of that time a young and quite small bachelor was among the fifty-two odd cows of which the latter bull's harem was on that day composed. At 1.26½ P.M. this young bachelor was covering one of the cows, my attention being attracted to the fact, as on the previous days, by the peculiar voice of the bachelor. The affectionate way in which the cow treated the bachelor made it certain that she was in heat, yet although the bull came up close to them, and even "nosed" the bachelor, the latter's presence and actions did not seem to arouse his suspicions, and the bull paid no other attention whatever to him. Presently the cow left the bachelor, and at 1.30 P.M. the bull covered her himself, finishing at 1.35½ P.M. Meanwhile the little bachelor was in a state of great excitement and displayed a very great deal of interest in the proceedings, several times jumping up on the side of the bull. The bull, however, as before, paid absolutely no attention to him. At 2.5 P.M. this bull was again seen *in coitu*, and meanwhile another cow "nosed" him a little. At 2.13 P.M. this latter cow was mounted by apparently the same young bachelor right under the bull's nose. The bull paid no attention whatever to this poaching in his harem, but moved to the other end of his domain, while the little bachelor went on riding the cow until 2.21 P.M. The behaviour of the cow to the bachelor

showed that she was evidently in heat; the cow and bachelor were of about the same size.

The extraordinary thing about it all is that this bull (and so, too, in the case of other bulls) had no objection whatsoever to allowing young bachelors to enter his harem and cover his cows. Had, however, one of the large outlying half-bulls approached the harem, or even moved about in its neighbourhood, the bull would have been very excited, and would have roared incessantly, and have gone out to attack the half-bull. The mere sight of copulation, however, going on near a bull does not excite his interest in the least so long as it does not occur in ground which he claims for his own.

At the South rookery I did not see anything of this sort going on, and the larger of the two bulls was much more careful in keeping the bachelors out. All of the latter that I saw were, however, with one exception, very small ones, and mixed with the cows at the southern edge of the rookery.

It is thus evident that the sexual feelings of even the smallest bachelors are very strongly developed, and I can thoroughly indorse the remarks of Mr. F. W. True on this subject (see his Report for 1895). Even the small male pups have the testes in a very forward state of development, and by the 29th July, at the South rookery, I saw the little black pups acting to each other in a way that made it certain that their sexual feelings had already made themselves felt.

With regard to the mutual relationship of males and females, there is little to be said that has not been already included under some other heading in this article. That the cows are as little "dove-like" in their dealing with the bulls as with their own sex, I am able to state from personal observation, and I have seen an offended female bite a bull savagely and then leave him and go to another harem. For a short time, however, during the breeding-season, a feeling which almost appears to amount to affection exists between bull and cow, and is best observed in the cases where a single bull and cow are to be found sitting by themselves. They are then for a short time inseparable, but after the sexual feeling has been satisfied they become as snappish to each other as before. Such pairs of breeding animals are more frequently to be observed at the end of the season, when the older seals have left the rookeries and the young bulls and cows come on to the breeding-ground. The harems are then small, and frequently consist of one cow only.

I have already quoted observations tending to show that the animals do not separate until copulation has taken place more than once. A young bull and cow noted at Zapadnie rookery on the 7th August were still together and inseparable on the 9th. As the season goes on, the cows forsake the beach in constantly increasing numbers for the water in its neighbourhood, while the bulls retire to sandy or shingly beaches, where they can haul up free from domestic worries.

The rookery-ground is then largely occupied by pups and young breeding animals of both sexes.

The following detailed observations made on the South rookery will, I think, be found of interest. It is of course impossible to give a complete set of continuous observations for the whole season, since there were other rookeries to be visited, entailing long and often tedious journeys, in which I was greatly dependent upon wind and weather. Thus, on one occasion, it took Dr. and Mrs. Stejneger and myself six days to make the journey of 21 miles by sea to the South rookery from Nikolski, and during five of these days we were camped on the beach under our boat waiting for favourable weather. My notes have, however, been supplemented in many cases by observations made on other rookeries, especially on the Kishotchnaya section of the North rookery, where I spent several days (29th June to 3rd July) in close observation of the seals.

My first visit to the South rookery began late on the 23rd June and ended on the 26th June. There were then no bulls at the rookery and no bachelors. On the morning of the 24th there were sixteen females on shore, and their number was shortly afterwards increased by the arrival of three more from the sea, making nineteen in all on the beach. With these were eleven pups, and there was in addition a small lot of about fifteen seals playing in the surf outside the rookery. During my stay at the rookery the number of seals rapidly increased from 41 to 89.

The females at the South rookery might at this time have been divided into three classes, that is, those who were on shore, the majority of whom had pupped or were about to do so very shortly, those who spent their time in the surf outside the rookery, and a very small number of females who belonged neither to one nor the other of the above classes, but were engaged in reconnoitring the beach with a view to shortly landing. The members of this last class frequently landed for a short time and then went into the sea again.

It was very evident that the numbers of the females in the surf, as well as of those on the beach, were constantly being added to, chiefly during the night. When a female arrived first she appeared to join the ranks of those playing in the surf. With them she remained for an unknown period, and then came in to reconnoitre the rookery, probably landing several times in a temporary manner before finally doing so for the purpose of pupping. Probably, however, had there been a number of bulls on the rookery, such females, having once thus landed, would not have been allowed to leave again so easily.

Mothers and Pups.

The females on shore, certainly those who had pupped, seemed to move about very little, and my observations of them lead me to believe that they do not leave their pups for quite a considerable time

after they have been born. Each female who has a pup lies quite close to it for some days. If she moves her position she carries the pup with her, usually holding it by the back of the neck, but sometimes lower down the back. If the pup moves from her it is caught and pulled back to its mother's side. It is no wonder then, after such a close association between mother and pup in the earlier part of the season, if later on they can, and do, recognise each other among the multitudes of seals occupying a rookery.

On one occasion (26th June, at the South rookery) I saw a cow who had quarrelled with another cow, and had been defeated, retire out of the pod of massed seals carrying her pup with her, holding it by its back near the tail. Another cow seized the pup by its neck, and a tug-of-war ensued before the mother got off with it. Finally, before she got quite clear another cow carefully smelt the pup, evidently with a view to be sure that it was not her own. On another occasion (at Kishotchnaya, on the 2nd July) I felt almost sure that a cow whom I saw moving her pup did so in order to save it from the ponderous tramplings of a bull.

The little new-born pups are the source of constant squabbling among their mothers, and any attempt at familiarity on the part of a stranger is at once resented in the most savage manner.

Few points are, indeed, more striking in the character of the Fur Seal than the spirit of inconsistency which causes the cows to lie so closely huddled together on the beach that one of them can hardly move without disturbing two or three of her neighbours, and all, one would think, must be imbued with the most friendly and sociable dispositions; yet the slightest stir or familiarity on the part of a neighbour is resented with a fierce snap, and if a pup ventures to approach a strange female in mistake for its mother it is at once seized, savagely shaken, and thrown away—even killed—much as a terrier treats a rat. Yet Mr. H. W. Elliott has devoted some space to a description of the meek and dove-like character of these female seals!

Not only is any familiarity on the part of their own species resented, but I have seen a female hold a regular sparring match with a glaucous-winged gull (*Larus glaucescens*, Naum.) who wished to make a meal off some recent placenta, and the little blue foxes which sat as close to the seals as they dared were constantly being chased away if they ventured to approach a little too close to the rookery. Sometimes they pay for their impudence with their lives, and I have several times seen a blue fox chased away by a cow who thought it had approached too near to the rookery. In 1896, I found at Zapadni, Copper Island, the carcase of a young blue fox which had evidently been recently killed by some cow or bachelor, whose seeming meekness it had trusted too much, and had received in return a fatal bite in the neck.

The newly-arriving females were treated with equal want of courtesy. Their desire always seemed to get right into the middle of

the mass of seals already on shore, but whenever a new-comer approached the edge of a rookery she was received with such a series of snaps that in one case at least I saw a female go right round the mass two or three times before she could get in. When such a seal has at last got into the rookery her progress to a resting-place is one constant series of fights, as she scrambles over the backs of her sleeping sisters, and finds her course disputed by each one in her way.

At this early part of the season the number of cows on shore did not seem to be appreciably affected by the weather, and I do not believe they will under any circumstances leave their newly-born pups.

So too at Kishotchnaya from the 29th June to the 3rd of July the cows were constantly arriving in large numbers, yet during that time there was never any great number of them in the sea, only about enough, in fact, to account for the newly-arrived females. I do not wish to say that the cows never left their pups, but I am certain that very few did so, and the number going to sea was always very much less than that of those coming from the water. At this time they have little or no fear even of a man, and can be approached and photographed at any near range. Those cows who pup late in the season stick equally close to their pups, and I found a young cow at Palata on the 9th August who stood up to me as boldly as a bull, and allowed me to photograph her and her pup at a distance of only a few feet.

Exactly how long the cows stay thus on shore after they have pupped it is in the present state of our knowledge impossible to say, but a small amount of light is thrown upon the question by the movements of one of the South rookery cows, whose back was marked with flesh-coloured spots in such a manner that she was always easily recognisable. This cow hardly moved her position during the three days of my first visit to the rookery :—

24th June,	morning	First seen.
24th ,,	6 P.M.	Asleep with pup in same place.
25th ,,	morning	Asleep a yard or two from former position.
25th ,,	6 P.M.	Asleep with pup in same place.
26th ,,	morning	Still asleep in much the same place.
24th July	...	Not noted.
25th ,,	3 to 4.15 P.M.	Asleep near same position.
26th ,,	...	Not noted.
27th ,,	morning	Asleep near old position.
27th ,,	about 3.30 P.M.	Went away with some stampeded seals.
28th ,,	morning	Absent.
28th ,,	6.15 P.M.	Again ashore near old position.
29th ,,	10.8 A.M.	Asleep with other cows on rock to south of rookery.
29th ,,	12.15 P.M.	Asleep on small rock near rookery.
29th ,,	3 P.M.	Asleep on rookery near old place.
29th ,,	6 P.M.	Ditto.
30th ,,	...	Not seen.
2nd August	...	Ditto.

Probably the cows do not leave their pups until the latter are capable of moving about by themselves, and refuse to be controlled by their mothers. The young pups grow with great rapidity. At first they are very weak and feeble-looking, but they seem to feed a good deal during the first few days of their life, and already, on the 26th June at the South rookery, there was a distinct difference visible between the pups which had seen a week or ten days of life and the little thin new-born ones. By the 30th June, at the North rookery, a few of the little pups were independent enough to begin to collect together in little pods, and on the previous day I had seen one swimming in the shallow water on Kishotchnaya reef. A fortnight later, on the 13th July, the pups lay outside the harems of the reef in black patches, giving the rookery quite a new appearance, and causing its outline to look very irregular.

I think these little podding pups may fairly be taken as an indication of the time each mother stays on shore with her pup after its birth, as well as an index to the number of females on shore. I do not think any female left her pup until about the 29th June, and that it was not until ten or twelve days later that any appreciable number of them did so. I believe also that for some days after the female has thus parted from her pup for the first time she does not go to any distance from the rookery, but contents herself with short excursions to the outlying rocks, reefs, or kelp-patches, where she washes or plays away the hours, and probably also feeds. This is borne out by my observations both at Kishotchnaya and the Reef as well as at the South rookery.

At the latter rookery (from July 24 to 30, 1897) we could always account for so many seals that it is extremely unlikely that any great number of them travelled to a distance from the rookery in search of food. Yet that they *were* feeding I know for a fact, having on more than one occasion seen them spewing up undigested portions of their meals while on shore. Taking this fact into consideration, as well as the fact that seals are usually to be observed by vessels coasting between Nikoski to the south-west of Copper Island, when at a distance of from 3 to 10 miles from the shore, and that in that region fish are abundant, as evidenced by the abundance of birds, I believe that the nursing Fur Seal mother gets her food for some little time after the birth of her pup at no great distance from the shore, and only lengthens her excursions as the pup grows older.

In the end, however, when at last she does leave her pup to travel to the distant feeding-grounds at sea, she remains there so long, either sleeping or playing, that when she returns to the rookery her udder is distended with milk and her stomach empty.

On these occasions the seal-mother very often finds a little ravenous and half-starved pup noisily awaiting her arrival and eagerly demanding his dinner from all the other mothers he meets. These, one and

all, snap at him with great severity, and so he goes on until his own mother, landing on the beach, at once commences "baaing" for him, and the pup, if he is within hearing, recognises her voice and answers the call, and the meeting of mother and child is obviously one of mutual recognition and great pleasure. Sometimes, however, the foolish pups stray away to other ground, where their mothers have great difficulty in finding them, or perhaps do not find them at all, and, as no other mother will take pity on them and feed them, their little starved carcases, pressed flat by the flippers of their comrades, sadden the eyes of the visitor to the rookery.

Food.

It is a strange thing that scarcely anything can be found in the stomachs of the seals on shore, whether males, females, or any but the youngest pups. The reason seems to be a twofold one, namely that the seals commonly feed at such a great distance from the rookery that their stomachs are empty by the time they return to shore, and secondly, that, even if they feed at no great distance from the rookery, they seem to prefer to sleep off the effect of a heavy meal on the surface of the water, which they find no doubt a far softer and pleasanter bed than the hard rocks on shore. Thus even the older pups, if killed on shore, are usually found to have empty stomachs, and to get one with a full stomach a search must be made among those asleep in the water off the rookery.

The habit of feeding far out at sea is adhered to with strange persistence by the fur seals, insomuch so that the pelagic sealers have found them plentiful at sea in August off the Commander Islands, in localities distant from 100 to nearly 200 miles from the rookeries. Yet, except in the immediate vicinity of the rookery beaches themselves, seals are rarely to be seen in the neighbourhood of the islands, except perhaps in one or two favoured localities where fish seem to be abundant. At the Saranna river, which enters the sea at a distance of about seven miles from the north rookery of Bering's Island, great numbers of salmon are caught annually, yet it is said that the seals never interfere with the salmon and are never seen in the neighbourhood of the river's mouth.

It is not, however, an invariable rule that seals killed on shore have empty stomachs, for on 5th August 1896, while examining the bodies of some bachelors which lay on the killing-ground and had been killed during the course of a drive on the previous day, I opened seven stomachs, of which one alone was empty, the remainder being more or less full of a pink soup-like and nauseous-smelling liquid, in which were many eyes and a few beaks of squid, also a few strips of white flesh, either of fish or squid. One stomach contained a bit of salmon, and there were pieces of what looked like seaweed in others;

but it was difficult to tell exactly, as the contents of the stomachs were somewhat decomposed. This observation is of interest in view of the statement by Dr. Stejneger (Report p. 69) that he "was informed that once on the South rookery a flock of bachelors was so full of octopods that they vomited up quantities of these mollusks while being driven."

On the whole, however, the stomachs are almost empty, containing only a little mucus, bile, a pebble or two, some parasitic worms, and, perhaps, some fish bones or beaks of squid. These, the remnants of the last meal devoured by the animal, are usually regurgitated on the rookery grounds, whence a collection of fish bones may be made such as will give a clue to the food of the seals, and in which the Pacific pollak was found, as on the Pribilof Islands, to play an important part. At sea the contents of the stomachs are very different, and Mr. Lucas and I found many full ones (12 out of 26 examined) when cruising on the U.S. Revenue cutter "Rush" among the pelagic sealers in Bering's Sea. On this occasion I thought I noticed a connection between the full stomachs and the empty milk-glands, and empty stomachs (or those containing only a few fish bones) and full milk-glands, seeming to show that the mother-seals go to the sea with their milk-glands quite empty and then eat largely and sleep until their milk-glands are again full, which occurs about the time that their meal has been digested.

Not only do the seals cast up fish bones on the rookeries but deposit there parasitic worms and excrement and urine in great quantities, so that the rookeries are by no means pleasant places to tramp over: the rocks are often slippery and the odour always characteristic. Add to which the fact that on the Commander Islands at least the seals are infested by great quantities of a small dark fly, and it may well be imagined that it is often pleasanter to look at the seals from a distance than to walk among them.

I think it is to the urine that must be attributed the growth of yellow grass (*Poa* sp. ?) which first appears on ground formerly occupied by seals but deserted by them. Such grass had to me very much the appearance of that which springs up on the bare places where rabbits have been feeding on a lawn.

Summary of Statistical Results.

My statistical results show the following:—Assuming that the total number of pups on the South rookery from the 24th to 30th July was 530, that there was no appreciable increase in their number in that time, and that there were no pupless females on the rookery, then there were on the beach during a series of twenty observations a number of females which varied from less than 1 to over 59 per cent of the whole, and which was, within those limits, exceedingly variable,

the average number on shore at any one time being about 24 per cent, and the consequent average number of absentees from the beach about 76 per cent.

An almost equally variable number of females, whose minimum was about 17 and maximum about 68, with an average of over 37 per cent, was always to be found on the reef or on the rocks close to the rookery. As the pups also frequented these rocks in numbers, except at high tide, and were there met and suckled by their mothers, I am of opinion that these seals may be regarded as also having been on the rookery beach, and that the two lots together must be regarded as equivalent to the counts of seals made at any rookery (and there are many such on the Pribilofs) where the beach is not protected by outlying reefs or rocks. In other words, it seems that the percentage to be added to the number of seals on shore, in order to account for the total number belonging to the rookery, must be different according as the rookeries are protected or not. In the former case it would be much more than in the latter.

Adding the number of seals found on the beach to those on the reef and neighbouring rocks, it is seen that, although the items are so variable themselves, the total is more constant, never falling below about 26 per cent, or rising above about 85 per cent, and with a pretty constant average of about 62 per cent. In other words, the variability of the numbers of seals on shore or on the reef was due to the movement of the seals from one locality to another, and not to their departure from the rookery.

Besides this average of about 62 per cent of seals which were never absent from the vicinity of the rookery, and the numbers of which were ascertained in all cases by actual count, there was a further number who were never far away and always in sight. The numbers of these could only in a few cases be obtained by actual count, and must be, therefore, regarded as estimated only. The figures are, however, as likely to be under as over the mark. The numbers of these seals were also variable, falling once to nearly 2 per cent, and rising to above 62 per cent, and having an average of about 21 per cent.

Combining these figures, I find that there was no occasion on which I could not account for over 65 per cent of the total number of cows, that on one occasion I could account for over 90 per cent of them, but that these figures must be regarded as extremes, the average number of cows accountable for during a series of sixteen observations being about 83 per cent, and the average percentage of absentees being, consequently, about 17.

There would appear at first sight to have been a slight increase in the number of absentees while my observations were being conducted, but a closer look at my figures[1] shows that there was no day on which there were not at one time or another at least 83 per cent of the seals

[1] Which are too long to be printed here.

accountable for, and hence only 17 per cent away. The chief change was due to the fact that fewer cows seemed to be lying on the beach than before, but these lay on the rocks or reef or in the sea in the immediate vicinity of the rookery.

There are, I think, only two deductions which can fairly be made from the above figures, and these are either—

1. All the females had pups, and in that case there was no day up to the 1st August on which a percentage of more than seventeen left the rookery for any length of time, or—

2. If the percentage of females at all times absent from the rookery is to be here applied as on the Pribilofs, the obvious deduction is that there was an unknown and somewhat considerable percentage of the females which were without pups, and which, hanging about the neighbourhood of the rookery, made up the numbers of seals which were daily to be seen.

Movements of the Pups.

The movements of the pups seemed to coincide with the rise and fall of the tide. At low tide they followed their mothers out on the reef, and slept with them on the outlying rocks. The rising tide, however, caused the swell to break over these rocks, and even to send a small breaker right across the reef. The pups always retired to the shore before this breaker, and on the day of our most successful count (29th July, at 6.15 P.M.), out of a total of 529 pups counted by myself, and 527 by Dr. Stejneger, only three were in the water or off the beach.

On these well-protected rookeries the pups learn to swim rapidly, and although up to the 30th of July there were no pups at the south rookery who dared face the surf or the waters of the deep sea, there were on that date 370 out of 530 who were capable of swimming about in the shallow water on the reef. There can be little doubt that they here learn to swim by following their mothers out on to the reef, where the rising tide cuts them off, and they are then forced to use their flippers. One little pup which Dr. Stejneger and I watched on the 29th of July had evidently never tried to swim before. It was cut off by the advancing tide while sitting with its mother on a small rock on the reef. As the tide advanced, the pup tried to balance itself on the top of the rock in a seemingly most uncomfortable position. Presently the cow moved off, and the pup had to follow her into the shallow water, but it was only after some time, and when it was teased by some other pups, that it dared to put its head under the water, and when it did do so it swam excellently.

On the 30th July a good many pups at the South rookery were still afraid to go into the shallow water, as I saw when I went down amongst them to remove some dead carcases. They must, however, have progressed pretty rapidly in their swimming lessons; for, whereas

up to the 29th July the smallest number noted on shore at any one time was 217, on the 30th July, at 8.30 A.M., there were only 160; and on the 2nd August only about 70.

On unprotected rookeries, like, say, Sabatcha Dira of Copper Island, the pups are prevented by the constant surf from learning to swim until they are bold enough to face the breakers, and so they learn to swim slightly later. Still, at Sabatcha Dira, on the 7th August, I saw a pup accompanying his mother with ease and confidence among the heavy breakers then coming in. But this was an exceptional pup; the vast majority were afraid to face the surf at all.

As soon as the pups begin to swim they amuse themselves by playing with pieces of seaweed, and no doubt anything nourishing which they come across finds its way to their stomachs. This is no doubt a preparation for their winter feeding at sea. The earliest date on which I saw a pup playing with kelp was on the 29th July at the South rookery. On the same day I saw a pup follow his mother nearly out to the breakers before he allowed her to leave him. I cannot but think that the pups must, in the first instance, gain a great deal of their first knowledge of where their food may be found by thus following their mothers away from the rookeries.

By the middle of August (first noted on August 10, 1896) the pups show signs of moulting and assuming their grey coat, their heads especially presenting a very patchy appearance. Later in the season it is a frequent sight to see pups playing with sea weed or anything else which may come in their way, and in shallow water I have seen them nibbling at something at the bottom; and, on August 17, 1896, at Copper Island, I saw a pup with something in its mouth which looked remarkably like a fish. On the 6th September 1896 I shot a puffin (*Fratercula corniculata*) on St. Paul Island, which, unfortunately, fell into the sea out of my reach. Some pups which happened to be playing near at hand seemed to take an interest in it, and sniffed at it, but I did not actually see them bite it. Again, at the landing-place at St. Paul Island, a pup was seen by me pulling at a rope on September 20, 1896. This happened again on the 24th. On that day when I was standing at the same landing-stage, a pup came swimming by without seeing me, and finding one end of the same rope floating in the water, he began to pull and play with it like a puppy dog. Presently I began to pull the rope in towards me, and had actually brought him in a bit, before he noticed my presence, and took to his flippers with a surprised hiss.

Excavations on Puffin Island.

"The place of tombs,
Where lay the mighty bones of ancient men."

By PHILIP J. WHITE, M.B., *Professor of Zoology in the University College of North Wales, and Director of Puffin Island Biological Station.*

"AND we will row to that little island of which I cannot say the name, I like it so much, it looks so lonely, just broken off, as it were, from Anglesea."

The Isle of Glannauch, Ynys Seiriol, Ynys Lenach, Priestholm, or Puffin Island, to which Edna Lyall thus refers in one of her novels, are names which have been given, from time to time in the course of history, to the small island lying like a watch-dog at the eastern end of the Menai Straits. For upwards of a decade this island has been closely associated with biological inquiries of various kinds, and the descriptions and illustrations of it have rendered it familiar to many who have neither set foot upon it nor seen it.

Like some other islands of which we know, Puffin Island has its saint. Professor Herdman, in one of his clever sketches, represents this saint, Seiriol by name, as seated on the rocky shore of the island, contemplating with complacency and evident approval a small party of zoologists trawling from a boat.[1] No doubt the mystic was interested in the biological features of the island and its surroundings in so far as his earthly wants were concerned, but more than this it would be venturesome to surmise. However, as so much good biological work had been done under his auspices, as it were, I felt that it was only right and proper that some effort should be made to investigate the ancient seat of his activities. Mr. Harold Hughes, who has been associated with me in the work of excavation, about which I shall presently speak, has examined the scanty records relating to the island, and has furnished us with a most interesting history.[2] I can but touch on it here, and perhaps cull a few lines from his narrative. In the early years of the sixth century Seiriol erected his cell on the

[1] *Fifth Puffin Island Report*, 1892.

[2] *Puffin Report*, 1894 and 1895.

island, and took up his abode there with his religious brethren. These monks or religious brethren, and those who followed them through the centuries, were known as the "Canons of the Isle of Glannauch," becoming eventually "Canons regular of the Order of St. Augustine." The life of these monks, as recorded by Giraldus Cambrensis in his *Itinerary of Archbishop Baldwin through Wales* in 1188,[1] was a simple one. He says: "There is an island, of moderate size, adjoining and almost united to Anglesey, inhabited only by hermits, living by the labour of their hands and serving God. This is remarkable that, when any discord arises among them by the influence of human passions, all their provisions are devoured and destroyed by a species of small mice with which the island abounds, but, when the discord ceases, they are no longer troubled. Nor is it to be wondered at if the servants of God sometimes disagree, seeing that Jacob and Esau contended in the womb of Rebecca; by contention Paul and Barnabas parted from one another; the disciples of Jesus strove as to which of them should be the greatest: for these are the temptations of human infirmity. Nevertheless virtue often by infirmity is made perfect, and faith is increased by tribulation. It is said, moreover, this island is called in Welsh, *Ynys Lenach*, or the Ecclesiastical Island, on account of many saints whose bodies are buried here, and no woman enters this island."

What the mice referred to above were we cannot say, but no doubt we shall find some traces of them, unless they were merely creatures of the imagination. The only rodent remains that we have hitherto found are those of the rabbit and common rat. This rat was very abundant on the island, until a few years ago, when it was exterminated.

The island seems to have been a crown-land up to 1654, when it was sold by Queen Elizabeth to one J. Moore. In the grant this note occurs—"I know not of what compase the saide Ilelande is, nor the comodities thereof. This is the furst pticular made by me of the p'rmises for this sale 29 Ap 1564." Later the island passed into the possession of the Bulkeley family, in whose hands it remains to the present day.

The excavations made by us have been chiefly in the vicinity, and to the east of the old tower standing about mid-island. Several ecclesiastical buildings appear to have been erected from time to time, and this tower formed part of the priory which was in existence in the twelfth century. Round the tower, but at ground level, there are walls, some of which belonged to the priory, while others evidently surrounded portions of the burial-ground.

In 1893 I made the first excavation,[2] a trench some fifteen feet in length, by three feet wide, and about thirty yards north-east of the tower, at a spot said to have been part of the cemetery.[3] The limestone

[1] Powel's Latin edition, 1804. [2] *Puffin Report*, 1892 and 1893.
[3] Hopps, *Archæologia Cambrensis*, vol. xv. 1869.

was reached at a depth of three feet. In the mould, which consisted first of a layer of black earth, then of a layer of brownish earth, and lastly of a layer of brownish clay, there were, especially in the first layer, numerous bones and teeth of the ox, sheep, boar, rat, and rabbit, but no human bones were found.

I then made a shorter and wider trench about fifteen yards north-east of the tower. The soil here was about four feet in depth. As in the first trench, there was, to begin with, a layer of black earth, followed by a layer of sea-sand, below which there was a layer of brown clay. In the layer of black earth there were numerous fragments of human bones and teeth, and fragments of the bones and teeth of the animals found in the first trench. Immediately above the layer of sand a human skeleton was discovered with the feet pointing to the east. On passing through the layer of sand two skeletons were found, lying side by side, on the same level and a few inches apart, imbedded in the brownish clay. It was therefore clear that the burials had been made in two layers, one superficial and the other deep. In the latter no injured bones, or bones out of position, were found; whereas, in the former, besides the skeleton, there were many odd and injured bones, thus indicating that this layer had been used more than once for purposes of burial.

The next and principal excavation was made conjointly with Mr. Hughes, immediately to the east of the tower, on the spot probably occupied by the sanctuary of the priory. We also excavated in the floor of the tower itself. We commenced digging at the entrance of the tower, and worked outwards between two parallel walls extending eastwards from its sides. On removing about two and a half feet of debris, we came upon a wide stone forming the fore part of the threshold of the doorway. Deeper and to the east of this stone, and passing through layers of charcoal, burnt materials, and lime, to the depth of about eight inches, the thick walls of an enclosure,[1] about five feet square, were exposed. Further examination proved this to be an ancient tomb. Beneath some rough sea-worn slabs, and covered with shingle from the shore, lay, with his feet to the east, the skeleton of a man. As he was a large man, and as the enclosure, so far as its length went, was relatively short, he had been buried with the knees drawn up. Sir William Turner, to whom I sent the skeleton for examination, describes it as that of a man in the later stage of middle life, with a well-developed muscular system, a hyper-brachycephalic skull, and a good sized brain. Is it possible that these remains, occupying as they do the most important ecclesiastical site of the island, can be those of Seiriol "the Bright," of whom Matthew Arnold sings in his "East and West"? If so, this place of sepulchre might mark the position of his early cell, because, as old records show, holy men were occasionally buried in the oratory where they were wont to

[1] *Puffin Island Reports*, from 1894 to 1897.

worship. Whether, however, the remains were those of our saint or not, they are evidently those of a man of note in his day and generation.

Proceeding eastward with our excavations beyond the enclosure first spoken of, we exposed a somewhat roughly constructed sepulchral cist beside the wall on the left hand side, and resting upon the rock. When the covering slabs were removed a number of odd and broken bones belonging to several individuals were seen, and beneath these lay two skeletons, one above the other. Immediately to the right of this cist, and behind a rude headstone, another skeleton was found, and to the right of this yet another. These skeletons were not enclosed in any way, and like those in the cist, their feet were directed to the east.

Beyond these skeletons we have just found a low sandstone wall extending transversely between the two main walls within which our work at present lies. We have not traced it fully as yet, and what lies on its further side we have still to discover. In the debris within and without the tower several worked building stones were unearthed. Here and in the upper mould of the two other excavations, smoking pipes, dating from the reign of Queen Elizabeth to modern times, were brought to light, as were also fragments of Elizabethan bottles and comparatively recent gun flints.

In a small excavation which was made at the south-west extremity of the island some fragments of pottery, apparently Elizabethan, were found, and beneath these a number of sea-shells and burnt bones, while in the sandy soil to the north-west of the biological station, at a depth of two feet, a prehistoric flint was discovered.

The story of the island, from the time when this flint was used to the time when the biological station was established, is a long one. We shall endeavour to spell this story out, but in this short paper I have merely tried to indicate some points in connection with our task, which so far has been by no means a fruitless one.

Mr. F. W. Headley on Evolution.

By R. F. Licorish, M.D.

Mr. F. W. Headley is to be congratulated, from the Lamarckian point of view, on the soundness of his conclusions as to the course of organic evolution as expressed in the May number of this Journal. And yet, strange to say, I have to protest against *his* interpretation of Lamarck as stated therein. Lamarck never stated, nor did he intend others to believe, that evolutionary changes are brought about by means so simple as implied by Mr. Headley when he states in his article: "The idea that the crawling of bees or other insects over plants, or anything in the environment, can have produced flowers, is too great a strain on the credulity of an ordinary man," as an illustration of Lamarckian views. He says, "or anything in the environment," yet farther on in the article (page 362) he makes the environment play a somewhat different *rôle*, and he attempts rightly enough, so far as the explanation goes, to explain how it works. He says: "The environment offers to animals all that they require, and lets them take what they want in any way they choose." Now that is so, and it applies with equal force to plants. We should remember that the environment of plants includes all conditions capable of acting on them above the surface of the earth as well as beneath it. What Lamarck contended for was that plants are modified chiefly through their nutritive processes, and we can well assume that flowers were so evolved; changes in the nutritive processes leading to change in reaction to other environmental factors.

Now, so little has Lamarck been understood in this respect, that even one of Huxley's acumen and knowledge has been led by the misunderstanding to make statements absurd and misleading. In "Lay Sermons and Addresses," article "Origin of Species," Huxley thus writes: "It is curious, however, that Lamarck should insist, so strongly as he has done, that circumstances never in any degree directly modify the form or organisation of animals, but only operate by changing their wants, and consequently their actions; for he thereby brings upon himself the obvious question, How then do plants, which cannot be said to have wants or actions, become modified? To this he

replies that they are modified by the changes in their nutritive processes, which are affected by changes in their circumstances; and it does not seem to have occurred to him that such changes might be as well supposed to take place among animals." That plants cannot be said to have wants is rather a strange assertion from a scientist of Huxley's eminence, and the statement that it did not occur to Lamarck that changes in animals take place through their nutritive processes, as he alleges they do as regards plants, is a deplorable bit of gratuitous imputation for a great reasoner like Huxley to make, seeing that Lamarck was continually reiterating that fact. For certainly Lamarck's "wants" include the want of food, and if circumstances force animals to modify their method of feeding, a new habit will or may be contracted, leading gradually through heredity to modification of organs. Again, we see the misinterpretation of Lamarck in Mr. Herbert Spencer's "Principles of Biology," when he implies that the idea as to what induces organic change in the theories of Erasmus Darwin and especially Lamarck, is identical or very similar to the motive force implied in "Vestiges of Creation" and Prof. Owen's works whereas there is no real likeness, or, in fact, no more than is between the vitalist's theory of life and that of the physicists.

I agree entirely with Mr. Headley when he states that the guiding principle of evolution must be sought for in the organism itself; for that is what Lamarck ever maintained. Again, Mr. Headley states that the paths open in the evolution of species are limited. That is also true, and for the simple reason that they must follow the lines of function. Take up any work on physiology, and we soon learn why the paths of evolution are limited, for organic life depends on only a few great functions, viz., nutrition, including respiration, reproduction, and locomotion, all governed by the nervous system, and hence it must be on these lines—the great vital functions, as distinct from the special organic functions—that evolutionary changes are brought about when changes in environment lead to change in organic reaction in the formation of new species.

It seems to me we should look at organic matter as a *condition of energy, i.e.*, as, in a highly plastic state, capable of being modified either directly or indirectly according to the *exigencies* of the organism. Weismann now admits (a modification of his former views) that variations are caused by the reaction of the germinal protoplasm to extrinsic forces. But why does he not see that this reaction to extrinsic forces is not limited to embryonic life, but is continuous during the whole life of the organism, from inception of life to death, gradually decreasing, of course, in inverse ratio with the duration of life of the organism. We should thus be able to account not only for variations appearing at birth, but also for the inheritance of functionally-produced modifications.

That the course of organic evolution is gradual—one step in a

definite advance being the basis of the next step—is also a purely Lamarckian conception, although Mr. Headley attributes it to Eimer. As regards Mehnert's principle of development, summarised by Prof. Thomson in the May number of *Natural Science*, that, too, is Lamarckian, for Lamarck's work clearly makes out that all progress in organic evolution must be studied from the physiological or functional standpoint. Hitherto it has been studied almost wholly from the morphological point of view. But that this limitation is fallacious must be plain if we admit that "the function makes the organ."

That any course in evolution can be due to chance, and not to responses to environmental changes, is to me unthinkable, for, look where we will, consider what we may, law and order prevail in nature.

BARBADOS, W.I.,
May 25th, 1899.

Meteorology and Ethics.

In days whose distance from those of our enlightenment is not great when measured chronologically, though vast when estimated in terms of mental modification, the organism's dependence on its surroundings was unrecognised, and man was master of his fate. But we have changed all that,—the organism is now a whirlpool in the sea of life, and, "man is being recognised more and more as a creature of his environment, a sequence of personalities, each one of which varies from all the others as the conditions of that environment vary." Instead of *coelum non animum* we read *coelum ct animum*, and the days of the study of the personality *in vacuo* have passed away for ever. And so we react from a false abstraction to hardly less obvious exaggeration. Flowers shaped insects' mouth-parts and insects formed the curves of flowers, the popular Lamarckian says, in the exuberance of his confidence in modifications and their heritability; and as for our vices, it is the fault of the weather. The environment, in short, has to serve its turn as the scape-goat of the human camp. But just as there was truth in the old doctrine of the organism's independence and man's mastery of circumstances, so there is truth in the modern reaction; and we have read with great pleasure, which we wish others to share, Professor E. G. Dexter's clever and careful essay on "Conduct and the Weather: an inductive Study of the Mental Effects of definite Meteorological Conditions" (*Psychological Review*, Monograph No. 10, vol. ii. 1899, pp. 103, 14 figs.). We hope no one will be unkind enough to recall the line "For now, these hot days, is the mad blood stirring"—it may be cool enough before this note is published—for the thesis which we would report on is no *jeu d'esprit*, but a sober induction.

The meteorologists are probably too busy with the affairs of their own young science, to care as yet much for the inspiration which comes from a contact with other disciplines; yet if there is one thing that the history of science teaches clearly, it is the value of interactions between the various departments of scientific inquiry. That meteorology touches biology at every corner is well known, for whether we study Palolo or the Plankton, migration or the mammoth, whether we take up Bonnier's recent studies on alpinisation or Clement Reid's newly published essay on the origin of the English flora, we have to

utter to the meteorologists the almost proverbial cry of the men of Macedonia—"Come over and help us." It is possible, however, that there may be meteorologists wise enough, ignorant enough, and humble enough to be assured through the medium of *Natural Science* that their data have a profound bearing on Ethics.

Our author tells us that "the modern science of Meteorology, emerging from the mist and darkness of ignorant guess and surmise has left its path strewn with many a shattered idol. Jupiter Tonans the Thunderer, Pluvius the Rain-maker, and a hundred other weather-gods were toppled from their lofty pedestals ages ago, while St. Swithin and his two-score of saintly colleagues, whose days dominated the weather for the rest of the year, have been quite as surely if more recently dethroned by the delicate instruments and skilful calculations of the modern weather-man." But the dethroning is evidently to be followed by an enthroning, and le roi qui vive is Weather. Quietly but firmly it dominates us all,—how effectively, it is the business of Mr. Dexter's essay to show.

It is of course a familiar saying and saving-clause of the physician that this or that is due to the weather, and he has accumulated here and there no small basis for his platitude. But mental states, especially emotional states, are affected, through the medium of the body, by the conditions of the weather, and thus the connection between meteorology and ethics is securely established. Indeed, it is generally recognised, though its inductive elaboration has been hitherto neglected. "There are many persons who are simply victims of the weather." "How inconsiderate are our friends when the east wind blows and the skies are heavy." "How dangerously doubtful seems to-day the venture which yesterday, in the bright sunlight, seemed certain of success." We have already detected the influence of the weather in the pages of our journal.

The poet as well as the physician has recognised the dominance of weather-influence; as hyperaesthete he feels it more keenly than most; as seer he has, as in so many other instances, the right of priority over science in the discovery which Mr. Dexter expounds. Although many may not accept the utterance as authoritative, it is of interest to note Byron's remark—"I am always more religious on a sunshiny day." But even more convincing is Southey's complaint, made during one of his visits to England after a long sojourn in Italy—"I miss the sun in heaven, having been upon a short allowance of sunbeams for the last ten days, and if the nervous fluid be the galvanic fluid, and the galvanic fluid the electric fluid, and the electric fluid condensed light, zounds! what an effect must these vile, dark, rainy clouds have upon a poor nervous fellow like me, whose brain has been in a state of high illumination for the last fifteen months." Professor Dexter also points out how the plot in Romeo and Juliet hinges upon the weather. What a wealth of meaning there was in Benvolio's apparently simple remark—"The day is hot."

But we must remember where we are and the solemnity of facts, and state the problem. *Have the various meteorological conditions, ringing in as they do combinations innumerable, a definite causal relation to human conduct?* Does the ever-changing weather present conditions in which impulse to action is more liable than usual to overcome an ordinarily overpowering inhibitory force?

The prôblem was attacked in two ways: "first, by the tabulation and discussion of a questionnaire sent to nearly two hundred teachers of all grades, from the kindergarten to the high school, superintendents of asylums and reformatories, and wardens of prisons and penitentiaries;" second, by an inductive study of several hundred thousand data correlating weather and conduct. It is evident that the possible fallacies are so numerous that a large body of results were necessary before any reliable conclusions could be drawn, and it is for those accustomed to statistical inquiries to say whether Professor Dexter's industry was or was not sufficiently prolonged to allow of the elimination of errors. However this may be, he certainly has not spared trouble in seeking to substantiâte his thesis, and Mrs. Dexter also shared in bringing the immense labour of tabulation to a successful issue.

It should also be recognised that the author does not take any crude or easy-going view of his problem. He has realised the complexity of the factors which influence conduct, and the difficulty of analysing out those which may be called meteorological. As an instance of this, we venture to give a quotation—one of the many pleasant interludes in his serious argument.

"The idea that the prevalence of suicide in this country (England) is due to our bad weather is precisely one of those hasty and illogical inferences which are characteristic of the Gallic mind. The constant gloom of bad weather ought to acquaint us so thoroughly with moods of depression that suicide would never occur to us. Look at Scotland, for instance, where suicides are rare. Why are they rare? Simply because a succession of Scotch Sundays has so accustomed the people to prolonged despondency, that any sudden misfortune cannot sink their spirits any farther. One has only to spend a dozen Sundays in Glasgow or Edinborough (*sic*) to become inoculated against suicide." . . . . As Dexter says, there is truth beneath the jocular vein of this quotation.

The results of the study lead to the following five conclusions:

I. "*Varying meteorological conditions affect directly the metabolism of life.*" Some of the conditions accelerate the oxidising processes of life, while others retard them; the former are called by the author anabolic, the latter katabolic, and we would accent his hesitation in using these terms, with the remark, that he thereby darkens his counsel with words without knowledge. Any other terms would have done as well, for no others could be worse.

II. *The 'reserve energy' capable of being utilised for intellectual processes and activities other than those of the vital organs, is influenced*

to a marked degree by meteorological conditions." Again we must demur most emphatically to the quasi-physiological expression which the author uses in summing up his results. His conception of "reserve energy" is a reflex of a commercial environment, and appears to us quite inapplicable to the real business of metabolism. It is an unconscious 'materialism'—an attempt to give a false simplicity to the facts.

III. "*The quality of the emotional state is plainly influenced.*" "It is safe to say that high conditions of temperature and humidity, cloudy and rainy days, and for many people high winds, are generally productive of more or less negative emotional states; while moderate and cool temperatures, low humidities, mild winds, and clear days are usually positive in their effects." But, as the author says, this thesis must be defended by means of an analysis based solely upon introspection; and though he tries to connect it with his doctrine of "reserve energy" he is not certain about it, and it is just as well.

IV. "*The reserve energy and the emotional state are both factors in the determination of conduct.*" Here the author seeks to show that his theory of "reserve energy" accounts for the discrepancies which are apparent on the supposition that the emotional state is the only factor.

V. "*Conduct, in the commonly accepted sense of the term, Death and Intellectual and Physical Labour bear very different relations to reserve energy.*" "As a conclusion, it would seemingly be safe to say that of the activities (or cessation of activity) possible to human beings, some are the result of excessive vitality, and others of deficient states;" and that, generally speaking, "those misdemeanours which have been classed under our study as those of conduct are the results of the former, while death is an accompaniment of the latter."

As it seems to us, the conclusion of the whole matter is that the author has brought forward strong evidence to substantiate the thesis that there is an indirect causal nexus between weather and conduct. But we do not feel sure of anything else in his results, and particularly we would respectfully suggest to him, that he has departed from the scientific method by mingling with his inductive results a physiological theory which is probably erroneous and certainly unnecessary.

X.

The Comparative Chemistry of our Forest Trees.

By P. Q. Keegan, LL.D.

By the chemistry of trees is meant not the special detection and demonstration of the chemical forces which exert energy within the living arboreal organism, but rather the detection and assignment of such separable and distinctive organic and inorganic bodies as are incidental to the vital processes thereof, whether these bodies furnish the stroma of the actual life, or are merely bye- or waste- products of the spent and exhausted activities. The tree, indeed, may be regarded as the outward and visible sign of an inward and wholly invisible force. The capital force is the mysterious one called "vital;" but chemical forces and their visible or detectable products, which here alone concern us, are set agoing thereby, and are manifested as a heritage or inevitable consequence. Nevertheless, it is absolutely certain that some of the most brilliant, beautiful, and distinctive constituents of the tree—of its stem, leaf, and flower—are not the results of any chemical processes known to us, and cannot possibly be artificially reproduced by the most capable and dexterous application of the latest and most approved synthetic methods and expedients.

The arborescent forms of the forest flora of the British Islands are not very numerous, but (native and denizen species included) they are sufficiently varied to render an account of their chemical constituents exceedingly interesting and instructive. If, for instance, we desire to study the chemical characteristics of the Gymnosperms, we can forthwith fasten on that stately and sombre-foliaged tenant of our upland wastes and craggy mounds known as the Scotch Fir (*Pinus sylvestris*). Perhaps we have been accustomed to consider the leaf as the most vigorously active of the vegetable organs, but here we see that a mighty portion of the energy is delegated to the woody tissues. For what is the meaning of the resinous matter which is so characteristic a constituent of the Coniferae, and the origin of which has been the theme of such acute and prolonged controversy? Some specially active mother-cells containing an opaque plasma, and situated in the external heart-wood, divide and divide again with great energy, separating from the adjoining tissue, and forming four to eight or more daughter cells

which split asunder internally, leaving a hollow space (resin-passage) into which there flows the product of their spent and exhausted labour (destructive metabolism), viz. the resin. Physiological operations of this very pronounced and particular nature are rather rare in the woody tissues of the stem and root of our Dicotyledons. Then again, we can attest the curious transformations which the starch, fatty, and resinous constituents of the wood of the Scotch Fir undergo at the different seasons of the year. According to Fischer there is no starch at all in the wood, pith, or bark during the winter; and Jonssen asserts that at this season the wood is entirely devoid of starch in all parts, but bears a considerable quantity of fat-oil, finely distributed, which disappears in April, while during the summer the wood is very poor in fatty matter. The needle-shaped evergreen leaves, again, are divested of starch in winter; but about the 1st April, even while the chlorophyll is still in the wintry condition, and although a low temperature and no special sunlight may occur, these organs are found crammed full of starch. So that here a very remarkable phenomenon is presented, viz. a plenteous production of starch following quickly on the winter sleep, and under conditions the very reverse of those which, in most of the dicotyledons of our latitudes, are indispensable for accomplishing a precisely similar effect. In fact, certain still undetermined causes, operative after a kind of pre-ordained periodicity, seem to dominate the physiological action of the protoplasm of these extraordinary foliar organs. Coniferous leaves are always much poorer in nitrogenous and in mineral constituents (ash) than those of deciduous trees, and the ash generally contains larger amounts of magnesia, iron, and silica. On the whole, it may be concluded, from a study of the character and quantity of the chemical constituents, that the coniferous Gymnosperms are subject to a fitful periodicity of physiological energy, interrupted by corresponding and longer periods of repose akin to hibernation, which permit of extensive accumulation of "dry substance" in the tissues under the form, more especially of the products of de-assimilation (tannoids, tannins, glucosides (coniferin), resins, waxes, and volatile oil), while on the other hand the products of assimilation (starch, fat-oil, and nitrogen-compounds) are relatively and absolutely scanty.

Reviewing now the more extensive and familiar field of the Dicotyledons, we are impressed not only by the comparative chemical similarity of certain of the woodland organisms, but also by the fact that a few other groups stand forth singly and, as it were, with an isolated heterogeneity as remarkable as it is apparently inexplicable. Peering adown the wondrous vistas opened out to us by the resources and appliances of chemistry, the squabbles of the "splitters" and "lumpers" of the would-be systematic taxonomists seem fantastic and puerile; the hair-splitting agreements or otherwise in the essential or unessential superficial characters of the organs of reproduction, etc., are liable to be contemned or wholly ignored. We find that species of trees very

closely related in systematic affinity are anything but very closely related as respects their physiological faculties, the sweep and potency of their vital energies, inasmuch as we can now attest and demonstrate that the inevitable chemical products thereof are, in the two cases, mightily different in quality and quantity. Bonnier has remarked that "the anatomical structure of a plant cannot always be deduced from its physiological functions; two plants, for instance, having similar chlorophyllous tissues may have very different powers of assimilation, and plants are known which have a palisade tissue more developed than others, but which, nevertheless, possess much feebler chlorophyllian functions." But where morphology fails, chemistry braces up in aid; and yet with all its magnificent powers and abundant resources it does not presume to be able to explain why or how it happens that one or two of our heath and forest species of the extensive order Amentaceae should be pre-eminent producers of fatty matters, leaving the rest shivering, as it were, in the cold of a lavish receipt and a thrifty expenditure of carbohydrates. I will now briefly pass in review the principal chemical features and characteristics of the dicotyledonous forest flora of our country.

The various species of Elm (*e.g.*, *Ulmus campestris* and *montana* and their varieties), in conformity with their lowly systematic affinities, exhibit nothing very advanced or developed, but rather a kind of degradation in the direction of a very facile production of that *bête noire* of the plant analyst known as vegetable mucilage. In the cortex special sacs evolved from the meristem, and due to a destruction of living cells with formation of cavities or canals, contain mucilage in large quantity; it is a pectosic mucilage with acidic function, being coloured by basic dyes; it swells up and almost wholly dissolves in water, but is not derived from cellulose. Some resin occurs in elm bark and wood parenchyma, but the quantity of tannin, phloroglucin, etc., is decidedly scanty in all parts. The leaves contain much carotin, considerable wax, and a little fat, and their starch-producing power is undoubtedly vigorous. In fact, the Elm is a very distinctive and decisive starch-tree, exhibiting a protoplasmic concentration rather uncommon; the lavish fortification of its bark and leaves with lime and silica, and the ability of some of its varieties to form a primary, persistent periderm, though only feebly suberified, are features clearly suggestive of the special quality of its activities.

Passing on now to these interesting morphologically allied congeners the Birch and the Alder, we realise in a striking degree the supreme value of chemical analysis in its application to botanical science. These two species are closely related taxonomically, and yet when chemically investigated we almost immediately discern very serious differences in respect to physiology. Both are fat-trees, *i.e.* during the winter no starch is found in the pith, wood, or bark, or in other words, their leaves are incapable of producing much starch, and the amylaceous

reserve is feeble and readily exhausted. So far they agree, but in the Birch the process of de-assimilation is not so complete as that in the Alder. In the former it is not pushed much beyond the lavish production of colourless waxes, resins, and volatile oils, and hence the outcome of the tannins, phlobaphenes, pigments, etc., is considerably restricted. The result is, that in the "queen of the woods" we have a silvery whitish bark with about 30 per cent white resin (betulin) approaching a wax or camphor in character, and only about 5 per cent tannin (all too feeble to impart a crimson coloration to the autumn leaves), together with an amount of phlobaphene too small to overpower the predominant suberification. The bast of this tree exhibits considerable lignification, but it is clear that the phellogen is perhaps the most active formative tissue in the entire rind. The case is pretty much reversed in the marsh-loving Alder wherein de-assimilation seems to reach its highest intensity. The bark of this tree sometimes contains as much as 20 per cent of a tannin which is highly carbonaceous, and very readily forms high red-brown and muddy shaded anhydrides of an eminently antiseptic character. The tannin penetrates freely into the medullary rays, parenchyma, and pith of the wood (it is very sparse in birch wood); in fact, without a doubt the Alder, taken all in all, is by far the most richly tannin-bearing of all our forest trees, and this constituent is of such a character and composition that it subserves the purpose of lignification rather than of embellishment, for as a chromogen it is useless save for colours dark and dun. The leaves contain a darkish brown oily matter, while the bark of the twigs encloses a bright yellow pasty mass of fat, wax, and a trace of volatile oil; carotin is very scarce even in the leaves. Cells filled with a homogeneous phlobaphenic matter seem mostly to replace or represent the highly suberified periderm of its congener the Birch.

The members of the sub-order Cupuliferae, viz. the Oak, Spanish Chestnut, and Beech, are more closely allied in chemical respects than the two foregoing species. No member of the vegetable kingdom has been more thoroughly and exhaustively investigated than the Oak. The peculiar shape of its leaves is no pledge of their physiological faculty, which is extremely powerful. The amount of starch which this tree produces and stores up (there is 37 per cent in the acorns) is, I think, considerably greater than that of any tree in our woods. A very distinctive variation is, however, observable in the Beech, where even in January and February the wood is very rich both in oil and starch, every cell of the parenchyma in the outer rings being full of the latter (which is not the case in most starch trees), and this predominance continues up till April when the wood is found still to be rich in oil (in fat-trees generally there is little oil in spring or summer). In fact, the Beech, chemically speaking, is a peculiarly eccentric organism. Even in its most massively developed trunk there is no marked distinction between the heart-wood and the

splint-wood; the wood-elements seem only very slowly to become completely lignified, and although the ratio of "incrusting matter" therein is ultimately extremely high, there exists only a very small quantity of tannin and that only infiltrating the walls; in the inner rings there is a specially abundant store of starch laid up to meet the tremendous drain of the "seed-year," this starch gradually changing into drops of wood-gum (xylan). Moreover, it requires more nitrogen than most other trees, and needs a plentiful supply of potash. The external economy, too, is as remarkable as the internal. The cortex is a veritable curiosity. "The whole tree," says Wicke, "sticks, so to speak, in a siliceous coat of mail, the silica forming a thick solid crust over the whole stem and the young twigs." The bark is said to contain 70 to 90 per cent of oxalate of calcium. Beech leaves are eminent for their large percentage of fatty matter, fibre, lime, silica, and manganese. In view of the considerable amount (some 25 per cent) of oil in the nut, the enormous affluence of starch, and the poor 2 per cent of tannin in bark and leaves, we can have no hesitation in pronouncing the Beech to be the most vivaciously active and powerful assimilating organism of our woodlands. Finally, how it happens that the Spanish Chestnut should specifically and exclusively produce the particular tannin called gallotannin in the bark and the wood (each contains about 7·5 per cent, the leaves about 6 per cent), is one of the mysteries shrouded beneath the impenetrable and inscrutable veil of forest secrecy.

Passing by the Hazel, Walnut, etc., which are not strictly speaking forest trees, we now approach a mystic tenant of the woods, a true native, and abundantly familiar, but which challenges the utmost possible chemical consideration that can be bestowed upon it. This is the common Ash (*Fraxinus excelsior*), and no lynx-eyed acuteness is requisite to enable anybody to perceive that even exteriorly it differs immensely from its arboreal neighbours and confreres. The smooth olive-grey bark, the astonishing knotty protuberances of its bursting flower-buds in spring, the almost absolute freedom from any intrusive or brilliant colorific effect in any of its members or organs, are so many tokens and pledges of characteristics entirely uncommon. It is a starch-tree, but its seeds contain 16 per cent of oil and no starch, and, moreover, on analysis one finds in the various organs such a considerable amount of waxy, resinous, and fatty matter, and such evidences of a facile decomposition of such carbohydrates as are produced in its leaves, that its claim to enrolment in the order Oleaceae is seldom questioned and never belied. In 1840 Gmelin had noticed a peculiar iridescence among the constituents of the bark of *Fraxinus Ornus;* but in 1856 Salm-Horstmar discovered a similar fluorescence in the infusion of the bark of *F. excelsior*, and in the following year he isolated, examined, and called it *fraxin.* Its dilute aqueous solution exhibits by reflected daylight a strong blue or blue-green fluorescence

which is destroyed by acids and increased by a trace of alkali. Fraxin is a colourless crystalline glucoside of a feebly bitter taste, and seems to be related to quinic acid or hydroquinone. The tannin of the Ash is totally different from that of any of our native or denizen trees: it is distinctly iron-greening, is not a glucoside, does not yield anhydrides by the action of acids, but only by heating dry or by repeated evaporation of its solution, when brown substances (recalling the dun shade of the autumn leaves) are produced, and finally on potass-fusion it yields protocatechuic acid but no phloroglucin. In fact, it is doubtful if any constituent with a phloroglucin nucleus occurs in the entire organism; for the quercetin found in the leaves from birth till late in August shows at all times reactions more like those of a tannin than of a mere tannoid compound. The leaves may be regarded as among the wonders of British botanical chemistry. Replete with chlorophyll and carotin, they contain much starch, fat, and resin, and from 6 to 9 per cent mineral matters (ash), but they are specially distinguished by the number and variety of decomposition products, which constitute an exceptionally high non-nitrogenous extract consisting of quercetin, tannin, inosite, mannite, glucose, gum, mucilage, malic acid and its calcium salt in astonishingly large quantity. On the whole, we see that the small and short-lived leaves of the Ash are extraordinarily active, and we are impressed by the apparent contradiction between the enormous percentage of mineral matters indicative of an intense transpiration and the small number (150) of stomata per square millimetre of epidermis; the carbohydrates produced on assimilation are largely oxidised to acids, but the chlorophyllian protoplasm itself in its descent on exhaustion stands hesitating, so to speak, on the first round of the ladder, the not very oxidised tannoids.

Much instruction and edification would doubtless be gained by a specific recital and description of the chemical constituents of the arborescent Rosaceae, *e.g.* the wild cherry, the rowan tree, etc., with their wondrous plethora of products of de-assimilation and of carbohydrate degradation; but as these are assuredly scattered and not forested, I now pass on to a tree which, although not a sterling native, has yet been frequently artificially planted in our parks and groves on such a plan and with such effect that the serried outskirts of a dense forest—vast columns upholding a dome of leaves and flecked with white clusters of blossoms, have at least been suggested. This is the beautiful Horse-Chestnut (*Aesculus Hippocastanum*), and truly there is something very satisfactory in the chemical distinguishment and examination of so many constituents that are comparatively simple and afford atomic groups more or less harmoniously proportionate. The well-known tannin, $C_{26}H_{24}O_{12}$, for instance, has a number of atoms of hydrogen nearly equal to those of carbon, and exactly double those of oxygen; hence its reactions come out very decisively, the deficiency in carbon being a great help towards the ready production of a series

of beautiful anhydrides, which never reach the humus-like, dull, dirty browns yielded by other tannins. The most striking constituent is the highly fluorescent aesculin described by Martius and St. George in 1818; it is related to the fraxin of the Ash, and this latter is also contained in the tree under review. In the bark a fluid oil, phlobaphene, and very small quantities of aesculetin and its hydrate, are also found. The leaves are eminent for their richness in carotin in early June, their abundance of queraescitrin (glucoside of quercetin), fat, wax, phlobaphene, and resin, and much tannin in autumn. The seed contains about 4 per cent fatty oil and 14 per cent starch, also fruit sugar, and a series of curious glucosides and bitter principles representative of proteid disorganisation. It is rather a remarkable feature that this tree and its allies exhibit very slight indications of the presence or decomposition products of gum, mucilage, etc.; they are all starch-producing trees, but apparently there is no superfluity, waste, or prodigality of this substance, and at the same time, and especially in some of the maples, there is an abundant deposition of waxy matters, and of siliceous incrustations. It is quite possible that some of the foreign species of Sapindaceae unknown to me may be practically fat-trees. On the whole, this order is extremely interesting; and coming away fresh from its analysis, we are impressed with the struggle, as it were, between the starch and the fat—the sugar rising into a supremacy, culminating in *A. saccharinum*, and with the lavish abundance and superb beauty of the products of de-assimilation.

One more tree remains to be noticed, viz. the Linden (*Tilia europaea*), which possesses morphological and chemical characters of extraordinary interest. It is the most pronounced fat-producing member of our woods. Its seeds contain no starch, and very little carbohydrate, but store up 58 per cent of a bright yellow non-drying oil. The wood seems to have some difficulty in parting with its reserves of fat, which remain, especially in the older rings, up till June or later, and the starch that creeps into its place begins to dissolve early in the autumn, none whatever remaining in the pith, wood, or bark during the winter. A special peculiarity of the tissues is the inconvenient abundance of mucilage both in the intercellular spaces of the parenchyma of the primary cortex and in the epidermis of the leaves. The large and very conspicuous sieve-tubes of the inner bast contain very thick, mucilaginous masses of albuminoid matters, but no starch. The amount of mineral matters in the leaves is very great, and in autumn they are incrusted with silica. On the whole this tree exhibits, except as regards starch, a very considerable energy of assimilation; and if some of its outcome tends towards decomposition or degradation, the proportion of the higher products of de-assimilation is decidedly not relatively high; in fact, those which depend on the destructive metabolism of starch are, under ordinary conditions, markedly absent.

Patterdale, Westmorland.

FRESH FACTS.

A Strange Dish. K. Kishinouye. "Edible Medusae," *Zool. Jahrb.* xii. 1899, pp. 205-210, 1 pl. 1 fig. Mr. Kishinouye of the Imperial Fisheries Bureau, Tokyo, has described two rhizostomatous medusae (*Rhopilema esculenta* and *Rh. verrucosa*) which are used for food in Japan. The animal is preserved with a mixture of alum and salt or between steamed leaves of Kashiwa, a kind of oak, with the application of slight pressure. To prepare the preserved medusa for the table, it is soaked in water about half an hour, then taken out and well washed, cut into small pieces and flavoured. It is easily masticable and furnishes an agreeable food. It is also used as a bait for the capture of file-fish (*Monacanthus*) and sea-breams (*Pagrus*). The latter are said to accompany shoals of the medusae.

An Early Cry. K. Fischer Sigwart. "Biologische Beobachtungen an unsern Amphibien. ii. Der Laubfrosch, *Hyla arborea*, L." *Vierteljahrsschrift Nat. Ges. Zürich.* xliii. 1899, pp. 279-316, 1 pl. From this entertaining account of observations on the "tree-frog" we select one note which is probably fresh. The observer has detected, quite apart from the breeding calls and the ordinary summer voice, a special strong cry of distress ("Angstschrei") uttered on an occasion of peculiar anxiety. As amphibians were probably the first vertebrate animals to find a voice, this observation of a cry of distress or alarm has peculiar interest.

What is the Difference between a Lake and a Pond? Otto Zacharias. "Ueber einige biologische Unterschiede zwischen Teichen und Seen," *Biol. Centralbl.* xix. 1899, pp. 313-318. The difference has hitherto been defined physically in terms of depth, etc. Thus R. Chodat, in his "Études de biologie lacustre," says that the minimum average depth for a true lake is 20-30 metres. But Zacharias shows that there are also distinct bionomical differences in the plankton, various algae, rotifers, etc., being dominant in ponds and sparse in lakes, and *vice versa;* and he substantiates this in some detail.

Artificial Production of Alpine Characters in Plants. Gaston Bonnier. "Caracterès anatomiques et physiologiques des plantes rendues artificiellement alpines par l'alternances des températures extrêmes," *Comptes Rendus Ac. Sci. Paris*, cxxviii. 1899, pp. 1143-1146. Continuing his experiments on this interesting subject, Bonnier finds that plants subjected to a daily alternation of extremes of temperature, tend to have more marked development of protective tissues, smaller and thicker leaves with a greater development of palisade tissue, frequent redness due to anthocyan, more assimilation per unit of surface, and relatively large flowers slightly less coloured than the normal.

Anal Glands of Dytiscidae. Fr. Dierckz. "Sur la structure des Dytiscides et le prétendu rôle défensif de ces glandes," *Comptes Rendus Ac. Sci. Paris*, cxxviii. 1899, pp. 1126-1127. According to this investigator the anal gland of *Dytiscus* is a unicellular gland facilitating the respiratory function

by secreting an oily substance which keeps the water out of the respiratory reservoir under the elytra. The defensive apparatus of which Bordas speaks is the rectal pouch.

FREEZING EGGS WITHOUT KILLING THEM. ÉTIENNE RABAUT. "De l'influence de la congélation sur le développement de l'oeuf de poule," *Comptes Rendus Ac. Sci. Paris*, cxxviii. 1899, pp. 1183-1185. Continuing experiments begun by his master, the late Camille Dareste, Mr. Rabaut finds that eggs exposed for half an hour in a freezing mixture at $-15°$ C. are not killed. Lasting perturbations are induced, and after warming (quickly or slowly) most of the eggs show in three days a proliferating blastoderm spreading over the yolk, but without trace of embryonic differentiation. Some showed abnormal embryos, and a very few—proving the individuality of the egg—were normal.

A SEXUAL PECULIARITY. A. KOWALEVSKY. "Quelques mots sur l'*Haementeria* (*Clepsine*) *costata*," *Comptes Rendus Ac. Sci. Paris*, cxxviii. 1899, pp. 1185-1188. In this leech there is marked protandry, and exchange of spermatophores occurs between the male organs at a period when the female organs are still rudimentary. Kowalevsky believes that the same phenomenon will be found to occur in other Hirudinea, such as *Piscicola*, the fish-leech.

EGG WITHIN EGG. FRANCIS H. HERRICK. "Ovum in Ovo," *Amer. Natural.* xxxiii. 1899, pp. 409-414, 3 figs. The occurrence of an egg within an egg is not a fresh fact; but it is often supposed to be. Mr. Herrick classifies the cases on record in two sets:—(i) enveloping egg usually normal, but occasionally of large size; blastoderm recorded in at least one instance; (ii) enveloping egg of colossal size, complete, with blastoderm probably present. One interpretation, which covers a number of cases, supposes that the small included egg represents a fragment of a normal ovum which has been ruptured in the upper part of the oviduct, or at least after the first layers of albumin have been added to the normal egg. It is possible that any substance which serves as a local stimulus to the upper part of the oviduct, whether coming from the ovary as abortive egg or egg-fragment, or from the duct as secreted product, may serve as a nucleus about which an egg-like body may be formed. Various inclusions which are not true eggs at all may be taken up by the egg and imbedded in it. But in other cases, such as double or triple yolk eggs, we have to deal with a fusion of the albumin in two or more ova, which are treated in the uterus as one egg and surrounded by a single shell. This process may sometimes be complicated by the inclusion of a third egg of normal size and already covered by a hard shell.

EXCRETION IN MOLLUSCS. L. CUÉNOT. "L'excrétion chez les mollusques," *Arch. Biol.* xvi. 1899, pp. 49-96, 2 pls. The injection method of studying the excretory function has led Mr. Cuénot to conclude that there are three seats of the process in molluscs:—(*a*) the nephridia, (*b*) closed cells isolated in the connective tissue or concentrated in the vicinity of the heart, and (*c*) in gasteropods, certain cells of the liver.

CEPHALIC EYES OF BIVALVES. PAUL PELSENEER. "Les yeux céphaliques chez les Lamellibranches," *Arch. Biol.* xvi. 1899, pp. 97-103, 1 pl. Pelseneer has now published a fuller account of the discovery, to which we previously referred (*Nat. Sci.* xiv. 1899, p. 6), and has given a plate. To what was then reported, we may add Pelseneer's note that the larval eye was seen in *Mytilus* and other forms by Loven (1848), and in *Mytilus* by Wilson (*Fifth Annual Rep. Fishery Board of Scotland*). Pelseneer has shown its persistence in various adults. As there was a misprint in one of our previous sentences, we may further note that the eyes do not make their appearance in *Mytilus* until after the formation of the first branchial filaments.

SOME NEW BOOKS.

THE SENSE OF HEARING.

L'Audition et ses organes. By Dr. M. E. Gellé. 8vo, pp. 326, with 67 figs. (Bibliothèque Scientifique Internationale). Paris: Félix Alcan, 1899. Price 6 francs.

This is a work of great interest, in which the author has brought together the results of modern scientific investigation on the structure and functions of the ear. It is divided into three chapters, the first dealing with sonorous vibrations, the second with the structure of the ear, and the third with auditory sensations. In the first there is a fairly complete discussion of the physical phenomena of sound—duration, intensity, timbre—but the application of Ohm's law regarding the composition of compound vibrations, and of Fourier's theorem to the analysis of curves, has not received much attention. It is impossible to obtain an adequate conception of the phenomena of hearing without the aid of these fundamental principles. The novelty of Dr. Gellé's book is that, for the first time, there is a systematic study of phonograms, or the tracings made on the wax cylinder of the phonograph. Many examples of these tracings are given from the writings of Hermann, M'Kendrick, Maragi, and Marichelle, in which the curious marks are seen, both as depicted by photography, as by Marichelle's method, and by graphic tracings, as recorded by the method of M'Kendrick. These tracings show many of the phenomena of tone to the eye of the observer; the number of the marks in a given time (or the duration of each mark) indicating pitch, the depth of the mark intensity, and the character or form of the mark quality or timbre. The interpretation of the curves, as photographed from above, is, however, much more difficult than that of the curves traced by a graphic method, and much yet remains to to be done. Dr. Gellé shows the marks or curves obtained from tracings of musical tones, as produced by various instruments, and also the tracings of syllabic sounds and words.

The character of a word is clearly brought out. It is a series of more or less explosive sounds linked together by vowel tones, each sound and tone having its own peculiar record of vibrations, the number of which depends on the length of time occupied in the pronunciation of each phone, or distinct and separate sound. Little has yet been done in the analysis of consonantal sounds and syllabic sounds, so that we may regard this department of phonetics as still in its infancy. The time may come when the educated eye, even from a tracing of nature's *long-hand* system of recording vibrations, will be able to recognise the word recorded; but at present that is impossible.

The only part of the second chapter calling for special notice is the elaborate description given of the deep roots of the auditory nerve. It is certainly remarkable that this nerve has more intricate connections with various parts of the encephalon than are possessed by any other nerve. As this is the case,

more especially for the cochlear division, the view is strengthened that this is the part of the nerve really connected with hearing, while the vestibular portion has to do with the transmission of the result of pressures connected with the sense of equilibrium and the position of the head (and perhaps the body) in space. True auditory impressions not only pass to their appropriate centres in the cerebrum but they may arouse, in a reflex way, many motor mechanisms, by their transmissions to the deep origins of probably all the motor cranial nerves. This striking fact suggests an explanation of how it is that music penetrates into the very roots of our being, and thrills us through and through.

At the close of the book, there is an interesting chapter on the results of pathological inquiries into the condition of the internal ear in deafness, and in cases of deaf-mutism. These results all support Helmholtz' theory of the analytic action of the cochlea. The real difficulties in the way of the full acceptance of this theory, namely, the perception of noise and the nature and influence of combinational tones, are not discussed.

The value of the book is lessened by the want of a good index. Altogether this is an excellent work, of a semi-popular character adapted for the perusal of any one who desires to know something of a fascinating subject, without having to plunge into mathematico-physical investigations. The latter, however, along with adequate anatomical knowledge, are the only means by which an accurate knowledge of the wonderful sense of hearing can be obtained.

JOHN G. M'KENDRICK.

SCIENCE AND QUARRYING.

Steinbruchindustrie und Steinbruchgeologie. By Dr. O. HERRMANN. 8vo, pp. xvi. + 428, with 6 plates, and 17 figures in the text. Berlin: Gebrüder Borntraeger, 1899. Price 10 marks.

This excellently printed work is, as its author is careful to point out, largely devoted to the stone industries of Saxony; but a general review of useful stones is also undertaken. The list of books helpful to the reader would astound a quarry-owner, but shows how the author is intent on putting forward mineralogical and geological knowledge as the true basis for the practical treatment of rock-masses. We miss, however, from this list Lévy and Lacroix's "Minéraux des roches," and the admirable tables of the same authors. While England is well represented, only three French works seem quoted, which is a loss when one considers the present brilliant position of geology and mineralogy throughout France.

The work opens with a modestly-written description of the common rock-forming minerals, stress being laid on the characters that make their presence welcome or unwelcome in building materials. An account of rocks then follows, based on Zirkel's text-book; but it seems unwise to introduce the question of geological-age at this late period into the classification of the igneous masses. What would a German quarryman think, were he imported into the Mourne Mountains or the Pyrenees? It is a pity, at any rate, to give grounds for the suspicion that geology is a matter of names, and of no value to the "practical tradesman." Pp. 83-150, however, should go far to show how minute structural details, or conditions of original deposition, such as those studied by the geologist, fundamentally affect the utility of rocks when they come to be placed upon the market. We gather from p. 180 that the growth of the artificial stone industry already affects the business of German quarries, and that the rates charged on railways are among the obstacles to progress. The same may be said with greater force of our own islands; and it is a question whether artificial stones, of uniform excellence, may not in time supersede natural ones for city use. This will only be a further example of science applying the tools of nature

to man's general advantage. The lightning-flash is, after all, an uncertain and unruly affair compared with the incandescent electric light.

For ornamental stones, however, it is doubtful if any artificial product should replace the natural; the question here is one of natural beauty as opposed to artificial colouring. Indeed, the startling breccias of some Italian manufacturers seem only parodies of nature. An artificial marble should be as impossible in architecture as an artificial flower-bed in a garden.

Dr. Herrmann's account of the marvellous variety of rocks in Saxony occupies 180 pages, and is followed by an appendix showing the choice of road-metal on Saxon highways. Would that we could echo—especially in Ireland—his conclusion (p. 351) that sandstone, limestone, dolomite, mica-schist, clay-slate, loam, and clay, while covering forty per cent of the surface of the country, are nowhere used as road material! When we see sand and turf-lumps thrown down on the denuded foundations of the fine old Holyhead road, as a metalling of modern days, we could wish for a little more of Dr. Herrmann's science mingled with our British practice.

This useful and agreeable volume concludes with a review of the public purposes to which the best known stones have been applied in various countries. It is a pity that the sumptuous use made of the "Irish green" serpentinous marble in recent work in Dublin could not have been included. The granite of Peterhead naturally comes in for mention, including references to Dublin and to Liverpool. The work involved in the preparation of this catalogue is not to be lightly estimated.

While some of the photographic illustrations are useful, others, such as those of stone-masons' buildings, are hardly in keeping with the work. A few bold pictures of wrought surfaces of stones, taken near at hand, would, to our thinking, be effective in a subsequent edition. G. A. J. C.

MORE APPLIED GEOLOGY.

Applied Geology. Part II. By J. V. Elsden, B.Sc. 8vo, pp. vi. + 250, with figs. 58 to 186. London: "The Quarry" Publishing Company, Limited, 1899.

This book is stated by the author to be written both for the geologist and the practical man. The second volume begins with chapter vi., which relates to ore deposits, and contains information of a rudimentary but well-chosen character, coupled with illustrations from various sources, notably from "Ore Deposits" by the late J. A. Phillips.

This chapter is represented by 19 pages of useful matter, illustrations included, and ends with a list of "Common Ores occurring in Mineral Veins," in which stromayerite and melaconite seem hardly common enough, in most localities, to deserve mention. Chapter vii. deals with non-metalliferous minerals. About 19 pages, including illustrations, are devoted to chapter viii., in which notes on prospecting, the recognition of minerals and their paragenesis, quarrying and mining are closely packed, somewhat to their mutual detriment. The four following chapters treat of building and ornamental stones, of these chapter ix. relates to igneous rocks, their modes of occurrence, structure, wearing, etc. On page 68 the reader's attention is arrested by a plan of Dartmoor, which, although it may embody a limited amount of truth, certainly demands an enormous exercise of faith from anyone personally acquainted with the borders of that granitic mass. In the section of the Worcestershire Beacon, Fig. 108, it might have been well to indicate the direction in which the section is drawn, and Fig. 110 appears to bear little or no relation to the adjacent letterpress. The definitions of rock-structures on pages 74 and 75 are in some cases far from satisfactory. The eruptive rocks are described in 14 pages, with some large, well-executed figures, representing their appearance in thin sections under the

microscope. These and certain other figures of microscope sections are, in some instances, rather diagrammatic, but are admirable of their kind. Chapter xi. deals with sedimentary rocks, and gives a short but useful description of sandstones and grits. Then follows chapter xii., describing limestones and slates, with several good illustrations.

Chapter xiii. is headed "Rocks used in the Arts and Manufactures." The reader may find some useful information here; but the two pages on gems might, for practical purposes of identification, just as well have been omitted. Chapters xiv. and xv. are devoted to questions of water-supply, drainage, landslips, tunnelling, road-making, etc. A map of England and Wales is given, showing the distribution of road-stones. It is difficult to say why the Land's End should be marked "syenite," and several additions might be made in other parts of the map; still it is a useful one.

There is an appendix on "Simple and Rough Methods for the Determination of Minerals and Rocks." Suffice it to say that they are simple and rough.

An index, in which Arkose precedes Architectural, and Bauxite comes before Basalt, concludes the volume, which, with its good features improved and its bad ones eliminated, may eventually fulfil the author's praiseworthy object in making it of use both to the geologist and the "practical man." In its present form it will probably better serve the purpose of the latter. The paper, the letterpress, and many of the illustrations are good. There are possibilities about such a book. The general plan of the work indicates a useful motive in a right direction.

F. R.

THE MUSEUMS ASSOCIATION.

Report of Proceedings, with the Papers read at the Tenth Annual General Meeting, held in Sheffield—July 4 to 8, 1898. Edited by Herbert Bolton. 8vo, pp. 193. London: Dulan and Company, 1899. Price 5s.

"The Editor," we read on p. v., "exceedingly regrets that so long a time has been occupied in completing these Proceedings, which, under ordinary circumstances, ought to have been in the hands of members and associates last October." What the extraordinary circumstances may be we are not informed; but among them may doubtless be reckoned Mr. Bolton's removal to Bristol almost immediately after his appointment as Editor of the Museums Association, and the mass of additional work connected with the rearrangement of large collections in the Bristol Museum and with the British Association Meeting, in which he thus became involved. Considering this, we do not think that Mr. Bolton need be greatly ashamed of having followed the example of previous editors in issuing the report eleven months after the meeting to which it refers.

We miss from the volume before us some of the papers which, according to the programme, were read at the meeting. Curators will regret the absence of Professor W. C. F. Anderson's stimulating remarks on "Museums in relation to Art Teaching," of the valuable suggestions as to "Methods of Preservation and Arrangement of Seaweeds for Exhibition" that came from Professor F. E. Weiss, and especially of the thoroughly practical "Note on some Arrangements and Fittings in the Sheffield Museum," read by the energetic curator of that institution, the enthusiastic secretary of the Association, Mr. E. Howarth. None the less, it would not have been advisable to have delayed publication of the report for the sake of including even these valuable contributions.

The contents of the report are of rather more varied nature than usual. The natural history aspect of museums has had prominence hitherto, but in the present volume are several contributions from the Art side. This is as it should be, for, different though the two branches appear, the curators of each can with profit exchange experiences and hints. Rather more art in the display of

natural objects, rather more system in the exhibition of things artistic, would often not be misplaced. Among the contributions to which we allude, special attention should be paid to that by Mr. James Paton, Superintendent of Museums, Glasgow, giving an authoritative account of the inception, establishment, and maintenance of the "People's Palace" in that city. The question of loan exhibits in museums is always a difficult one, and those who have had to consider it will read with amusement Mr. Paton's witty classification of lenders, and agree with him and Polonius that one should "neither a lender nor a borrower be." Mr. John Maclaughlan, of the Albert Institute Museum, Dundee, writes on "Sculpture in Art Museums," in a way that should be of much use to other provincial curators. Mr. William White's paper on "The Individuality of Museums" is chiefly devoted to an exposition of the Ruskin Museum, of which he is the curator. It is followed by "Practical Notes and Suggestions on Modes of exhibiting Museum Specimens," drawn from Mr. White's experience in the same museum; several of these are original and valuable.

Among articles that refer to all classes of museums, the place of honour is of course due to the address by the genial President, Alderman W. H. Brittain, who gives an account of the labours of the Museum Committee of the Sheffield Corporation. In a paper on "Provincial Museums and the Museums Association" Mr. H. Bolton suggests the compilation of a return of statistics as to the present condition of all museums in the United Kingdom. Such a statement would be of great value to curators, councillors, and educationalists, and we are glad to see that the Association has appointed a committee "to obtain information respecting museums on the lines of Mr. Bolton's paper," and that the General Secretary has been instructed to prepare a form to be sent to museums for their officials to fill up.

Mr. W. E. Hoyle's illustrated account of "The Electric Light Installation in the Manchester Museum" is thoroughly practical, and since that museum seems to have solved many of the difficulties incident to artificial lighting, this paper should be studied with care by all who propose to adopt the electric light for similar institutions. "The cleaning of museums" may seem an obvious duty, and it is just conceivable that the cleaning and dusting of the public portions of most of our modern museums is adequately carried out; but Miss Clara Nördlinger, of the Manchester Museum, cannot emphasise too strongly the need for "a judicious and efficient daily dusting of the workrooms used by the staff"; ventilation is usually lacking in such apartments, while the atmosphere is full of particles of arsenic, corrosive sublimate, and other poisonous and irritating substances. Such rooms are never properly cleaned, except, perhaps, in the Manchester Museum, and the health of the staff suffers in consequence.

Papers of more restricted range, and dealing chiefly with matters of natural science, are the following:—Professor A. Denny of Sheffield, on "The Relation of Museums to Elementary Teaching," which contains nothing more novel than common sense. Mr. E. M. Holmes, of the Pharmaceutical Society, writing on "The Arrangement of Herbaria," describes the methods adopted in various public establishments, and selects from them numerous useful suggestions. He favours the alphabetical arrangement for all small herbaria: undoubtedly it effects a great saving of time. In pursuance of this, he gives an alphabetical list of the natural orders of plants, with the numbers affixed to them in Bentham and Hooker's "Genera Plantarum," and with cross-references to the names used in Engler and Prantl's "Natürlichen Pflanzen-Familien." Dr. H. C. Sorby has yet another note on "Marine Animals mounted as Transparencies for Museum Purposes"; many of his beautiful preparations are to be seen in the Sheffield Public Museum, where they have been exposed to the light for several years without deterioration. Mr. Harlan I. Smith, of the American Museum of Natural History, suggests a detailed classification for "The Ethnological Arrangement of Archaeological Material"; it is thought that it may lead the

collector in the field to procure common objects such as he otherwise might overlook, and this seems to us a thoroughly valuable suggestion. Mr. S. Sinclair describes "The Australian Museum," of which he is the secretary. The last paper in the volume, by Mr. F. A. Bather, of the British Museum (Nat. Hist.), describes "some Russian Museums" visited by him when attending the International Geological Congress in 1897. The account of the Caucasian Museum in Tiflis has a timely interest, since its curator, Dr. G. Radde, has just been awarded the great gold medal of the Russian Geographical Society. Other museums described are those of St. Petersburg, Reval, Jurjev (Dorpat), Moscow, Saratov, Astrakhan, and Theodosia. The notes are mostly geological and zoological, and are followed by the drawing of a few morals, professedly referring to Russia, but peculiarly applicable to museums nearer home.

As usual, a few reviews and notes close the volume; but we regret to see that the Secretary has not furnished any report of the discussion following the papers. Such reports in former years, despite occasional verbosity, contained much useful matter that otherwise would not have achieved publication. We trust that this will be remedied at the next meeting, which we are informed is to be held at Brighton during the first week of July.

CRITICISM WITHOUT KNOWLEDGE.

Views on some of the Phenomena of Nature, as seen from the Workshop, the Factory, and the Field. Part II. By JAMES WALKER. 8vo, pp. 187. London: Swan Sonnenschein and Company, Ltd., 1899. Price 2s. 6d.

Mr. Walker is a paradoxer of the first water. His quarrel with modern science is partly verbal; but the greater part of his booklet is taken up with denunciation of the undulatory theory of light. He takes fright at the largeness of the numbers used to describe the number of vibrations per second in the motion that is the physical concomitant of what we call red light, and imagines that the writing out of these by numbers across a whole line of print is an argument against their existence. He has still to learn the truth that largeness and smallness are purely relative terms, and that the billionth of an inch is as truly a magnitude as the distance from the earth to the sun. It would be vain to attempt any criticism in a short notice. Enough to say, that his representation of the modern theory of light and radiant heat is a travesty, and shows extraordinary ignorance of the elements of wave motion. In support of this statement we give one quotation as a sample. In his description of the production of lightning *according to the science of to-day*, he says, "All, from every single molecule of that vapour, these motions and quivering waves of ether somehow drop the molecules, forsake them, abandon them; and although being nothing themselves but the simple quivers of ether, somehow collect themselves into a flash of an irresistible force of destruction, occupying not one-half of a cubic inch of space," etc. We congratulate our author on this very remarkable theory of the production of the lightning flash. It is his alone! It may be well to point out that, although Mr. Walker scoffs at scientific men for their gratuitous invention of the ether, he himself falls into the same pit by inventing "electrogene," which, so far as may be gathered from the vague references that are made to it, is a kind of material squirted out from the sun. To expose the fallacy of most of his arguments would be wasted labour. *Magna est veritas, et prevalebit;* and it is doubtful if tomes of argument could ever convince Mr. Walker of his sublime ignorance of the real basis of our ethereal dynamics.

C. G. K.

A HISTORY OF EXPERIMENTAL PHYSICS.

Geschichte der physikalische Experimentier-Kunst. By Drs. GERLAND and F. TRAUMÜLLER. 8vo, pp. xvi. + 442, with 425 illustrations. Leipzig: Engelmann, 1899.

To trace from their hazy beginnings the gradual and laborious development of what are now familiar and simple truths is always a fascinating study. If rightly pursued it should give us a psychological insight into the mental modes of man. One great difficulty must ever be the imperfection of the historic imagination. Just as the mature intellect is apt to misinterpret the modes of thought of the child or savage, so we, the heirs of centuries of accumulated knowledge, have difficulty in appreciating the intellectual needs and powers of our ancestors. Where, however, as in the case before us, the mark of the stage of culture arrived at is a mechanical contrivance or an illustrative experiment, there is less play for the personal equation, there is more chance for a sound judgment. Doctors Gerland and Traumüller have put together an extremely interesting book in which is presented, on its purely experimental side, the evolution of physical science from the early days of the Assyrians, Egyptians, and Greeks, through the times of the Middle Ages to the end of the sixteenth century, when with Galileo the modern school of experimental science may be said to have begun, and from this epoch on to our own days. Nearly a century before Galileo's time, however, we find in Leonardo da Vinci—famous even in his own day as painter, sculptor, musician, architect, and engineer—a type of the true scientific spirit. Particularly fruitful were his inventions and discoveries in hydraulics.

To give a fair notion of the contents of the book, suffice it to say that it is chiefly concerned with the invention of such familiar instruments as the telescope, microscope, pendulum, air-pump, thermometer, barometer, hygrometer, the electric machine, voltaic cell, galvanometer, induction coil, telegraph, etc.

The cuts and illustrations are numerous and instructive. Many are reproduced from original sources, and some are of high interest. Perhaps the most curious is the picture of von Guericke's experiment showing two teams of horses (sixteen in all) engaged in "a tug of war," the object being to pull asunder two gigantic Magdeburg hemispheres within which a vacuum has been formed. Very instructive also are the ingenious mechanical devices employed by our scientific forefathers to illustrate or demonstrate important mechanical principles. Not a few of these might with advantage be introduced for demonstrative purposes in our schools and colleges. C. G. K.

POPULAR ENTOMOLOGY.

True Tales of the Insects. By L. N. BADENOCH. 8vo, pp. xviii. + 255, with 44 figs. London: Chapman and Hall, Ltd., 1899. Price 12s.

IT was a happy inspiration of the author to devote most of this handsome volume to insects with stories of such interest and so little hackneyed as are those of the Orthoptera. Though popular in aim the book bears evidence of a true love of entomology and of a knowledge of the creatures described that are far from universal in similar works; and few readers will lay it down without the desire to learn more of its subject. The essays on Lepidoptera, which occupy the last eighty pages, are scarcely equal to the others.

Unfortunately the literary form often leaves a good deal to be desired. Such sentences as these are too frequent:—"Others again can fly, having ample wings, and, oddly enough, often gaily coloured. Look at the large spectre *Acrophylla titan* of Australia, a giant of its kind; its charming wings generally

blackish-brown in colour, but irregularly spotted and banded with white, the costal portion variegated with green and pink, and expand fully eight inches." "The colour of the body in many Phasmidae may change from brown in early life to green, subsequently returning to the brown tint. If this be owing to the presence of chlorophyll or other plant-juices among the insect-tissues, its explanation is not far to seek." "Sir John Hunter" is a slip that probably expresses admiration for his genius.

The illustrations of the insects deserve high praise, and the printer has done his work well. The book fills a place not previously occupied in the literature of entomology, and places within reach of English readers much varied information. The quaint forms and admirable disguises of the leaf-insects and "walking sticks," the methods of capturing prey employed by the mantis, the beauty of colour, the methods of producing sounds, and many other curious traits, are all described here, and should attract students to the Orthoptera, which rarely get the attention they deserve. J. H. W. T.

In the February number of the *American Geologist* Mr. W. S. Gresley throws some "Side-light upon Coal Formation," in adducing evidence that many coal-seams have not undergone any appreciable vertical compression since the time of their formation from decaying vegetation. He also points out that when coal arises from drifted deposits laid out in water, the shale band occurring above the coal may represent that which originally underlay the plant-remains. Such reversals by the agency of denudation, the materials of the highest original bed becoming laid down first in the new area of deposition, then those of the bed below, then those of the next bed, and so on, are of course not uncommon in the geological series.

Mr. J. B. Woodworth writes of the classification of glacial deposits, laying useful stress on the association of "sands and gravels" with the melting of ice-masses *in situ*. In introducing one or two new technical terms he, almost by miracle, avoids the use of Greek, a language which has preponderated in the modern geological literature of America, to the confusion and astonishment of Eastern readers.

Mr. Hovey's report of the winter meeting of the Geological Society of America contains a number of suggestive abstracts. Mr. Walcott's announcement (p. 99) of "plates of crustaceans closely related to Eurypterus" in the Algonkian beds of Montana, 4000 feet below the base of the Cambrian, will be received by palaeontologists with respectful watchfulness. Possibly the lover of thrust-planes will also want to have his say in the matter. At the present time students of variation in igneous magmas will read with interest Mr. Emerson's observations on absorption by granite, quoted on p. 105.

In the March number of the *Naturalist* Mr. O. Grabham gives an account of the bats found in Yorkshire, with notes on their habits in confinement. The absence of attention to recent emendations in nomenclature is as conspicuous in this as in an earlier paper on British bats noticed in these columns. Our own opinion with regard to such emendations is, that it is frequently desirable to "let sleeping dogs lie"; but that when they have once been made by a naturalist of recognised eminence it is the duty of humbler folk to follow suit, and not to presume to have opinions of their own on such subjects.

We are grateful to the editor of *Finland* for sending us a copy of the first number of his beautifully printed, admirably written magazine. The subjects with which it deals, though of enthralling interest, can scarcely claim to be touched on in a scientific journal, except in so far as every worker in science thereby confesses himself a lover and an advocate of freedom, education, and the right to know and think. The offices of *Finland* are at 106 Victoria Street, London, S.W., and the price is 3d. a number.

OBITUARIES.

RUDOLF LEUCKART.

BORN OCTOBER 7, 1822; DIED FEBRUARY 6, 1898.

It has been a matter of regret to us that no obituary of this great zoologist has previously appeared in our pages,—an omission mainly due to the busy pre-occupation of those best qualified to write such a notice. Yet we are not very far behind some of our contemporaries, for the May number of the *Zoologisches Centralblatt* furnishes us with the material on which this note is based.

Rudolf Leuckart was the son of a senator and printer at Helmstedt, and nephew of the zoologist Fr. Sigismund Leuckart. He studied at Göttingen, graduating as Doctor of Medicine in 1845, and was brought much under the influence of Rudolf Wagner, whose assistant he became. After a period of activity as privat docent he was called in 1850 to Giessen as Professor of Zoology in succession to Carl Vogt.

Even in Göttingen he had defined the characteristics of his future work:—(1) by numerous detailed researches, (2) by his generalising essay "Ueber Morphologie und Verwandtschafts-verhältnisse der wirbellosen Thiere," and (3) by helping H. Frey in preparing a second edition of Wagner's "Comparative Anatomy."

Soon after he had settled down in Giessen, where he remained till 1869, he published along with C. Bergmann a treatise which was at the time and still remains a remarkably strong piece of work—the "Anatomisch-physiologische Uebersicht des Thierreichs" (1852). His subsequent essays on polymorphism, division of labour, alternation of generations, parthenogenesis, and especially, perhaps, his article "Zeugung" in Wagner's Dictionary of Physiology (1855), were also notable contributions to the more general problems of Zoology.

In his detailed researches he ranged from Protozoa to Cephalopods, from Siphonophora to Pteropods, from the development of insects to that of the vertebrate eye,—indeed, over the whole animal kingdom,—but the department of study which seems to have fascinated him most, and in connection with which he is best known, was parasitology. To what is now known of the structure and life-history of Trematodes, Cestodes, Nematodes, Acanthocephala, Linguatulidae, etc., Leuckart made very important contributions, many of which were summed up in his famous work, "Die menschlichen Parasiten und die von ihnen herrühenden Krankheiten" (1863-1875). A second edition of this indispensable compendium was begun but, unfortunately, never completed. The first part is well known to students in this country by Mr. Hoyle's translation (1886, Pentland, Edinburgh).

In 1869 Leuckart was called to the professorship of zoology in Leipzig, and there he had wider scope for his enthusiasm and skill as a teacher. To name his students who have become famous would fill a page, and the splendid *Festschrift*

which was dedicated to him on his seventieth birthday affords eloquent testimony of the respect and gratitude of those who had the privilege of sitting at his feet.

The wall-diagrams by Leuckart and Nitsche are almost as familiar to the student as Leuckart's memoirs and his bibliographical Berichte (1848-1879) are to the investigator.

As generaliser, specialist, and teacher, Rudolf Leuckart was certainly one of the great zoologists of the century.

See BÜTSCHLI, O., *Zool. Centralbl.*, vi. 1899, pp. 264-266.

CARUS, J. V., Zur Erinnerung an Rudolf Leuckart, *Ber. Ges. Wiss. Leipzig*, 1898, pp. 51-62.

BLANCHARD, R., Notices biographiques. I., R. Leuckart. Avec portrait. *Arch. Parasitol.* 1898, pp. 185-190.

GROBBEN, C., Rudolf Leuckart. Ein Nachruf. *Verh. Zool.-bot. Ges. Wien.* 1898, 5 pp.

JACOBI, A., Rudolf Leuckart. Mit Porträt. *Centralbl. Bakteriol.* xxiii. 1898, pp. 1073-1081.

X.

The death is reported by telegram of Mr. JOHN WHITEHEAD, the well-known collector and explorer, who succumbed to an attack of pestilential fever while on a scientific mission in the island of Hainan. He left England in the autumn of last year to explore the less known islands of the Philippine group. On his arrival at Manilla, he found the condition of things too disturbed to permit of his going into the interior, and so made his way to Hainan, the highlands of which have never been traversed by European. Mr. Whitehead has during the last three years been engaged in the exploration of the Philippines, and by his work he added greatly to our knowledge of the zoology of the group. In his last expedition to the island of Luzon, Mr. Whitehead made an unexpected discovery in the shape of a new and peculiar mammal fauna inhabiting the Luzon highlands, and believed to be isolated on a small plateau on the top of Mont Data, in the centre of northern Luzon at an altitude of from 7000 to 8000 feet. As a collector Mr. Whitehead was highly esteemed, and his death at the early age of 43 will be especially felt in the Natural History Museum at South Kensington, the zoological collections in which have been enriched through his industry and skill.

The deaths are also announced of Prof. L. A. CHARPENTIER of the Faculty of Medicine, Paris; on April 20, at Montauban, Prof. CHARLES FRIEDEL (b. 1832), one of the most distinguished of French chemists, and one of the initiators of the French Association for the Advancement of Science; Dr. THEODOR VON HESSLING, formerly professor of anatomy in the University of Munich, at the age of 83 years; on May 6, aged 73, the Rev. T. NEVILLE HUTCHINSON, who was science master at Rugby from 1866-83, and did much to introduce the study of science in the English public schools; on May 17, the Rev. JONATHAN SHORT, vicar of Hoghton, near Preston, in his 74th year. He was well known as a geologist and antiquarian throughout the North of England, and has taken an active part in collecting and preserving the historical records of Lancashire.

NEWS.

THE following appointments have recently been made:—Dr. Howard Ayers, professor of biology in Missouri University, to be president of the University of Cincinnati; Dr. Tarlton H. Bean, to be director of forestry and fisheries of the United States Commission to the Paris Exposition of 1900; Dr. C. E. Beecher, professor of historical geology in Yale University, to succeed the late Prof. O. C. Marsh as curator of the geological collections of the Peabody Museum, and to be a member of the executive council of the museum; Miss Edith Chick, as Quain student in botany for three years at University College, London; W. R. Crane of Janesville, Wis., to be assistant professor of mining engineering at Kansas University; Dr. G. Davis, to be assistant professor of applied anatomy at the University of Pennsylvania; Dr. Ida Hyde of Cambridge, Mass., to be assistant professor of zoology at Kansas University; Miss A. Lambert, M.Sc., to be assistant lecturer in biology in the University of Melbourne; Dr. G. Lindau, to be Custos of the Imperial Botanical Museum of Berlin; Miss Lillie J. Martin, to be acting assistant professor of psychology in Stanford University during Dr. Frank Angell's absence in Europe; Prof. E. A. Schäfer, F.R.S. of University College, London, to be professor of physiology in the University of Edinburgh, in succession to the late Prof. Rutherford; Dr. J. L. Wortman, of the American Museum of Natural History, to take charge of the new collections of fossil vertebrata in the Carnegie Museum, Pittsburgh.

Mr. F. J. Bennett has resigned his position on the Geological Survey of England, after 30 years' service, during which he has mapped large areas of Cretaceous and later rocks in Surrey, Berks, Wilts, and the eastern counties.

We regret to learn that it was owing to medical orders that Prof. E. Ray Lankester was compelled to withdraw his promise to deliver the "Robert Boyle" lecture at Oxford this summer. He has been recruiting his health by a trip to various Continental museums. The Boyle lecture was delivered on June 6, by Prof. J. G. M'Kendrick, who took for his subject the physiological perception of musical tone.

On the occasion of the birthday of Her Majesty the Queen, the following among other honours have been bestowed:—a baronetcy was conferred on Prof. J. S. Burdon Sanderson, and the honour of knighthood on Dr. W. Mitchell Banks and Dr. John Sibbald. Mr. Stanley was appointed to be G.C.B., and Prof. Michael Foster to be K.C.B. Dr. J. C. Meredith, secretary of the Royal University of Ireland, is also among the new knights.

In a convocation at Oxford on May 16, the degree of M.A. (*honoris causa*) was conferred upon Mr. Roland Trimen, F.R.S., formerly curator of the South African Museum, Cape Town, and late president of the Entomological Society of London.

On June 21, at the Oxford Commemoration, the honorary degree of D.C.L. was conferred *inter alios* on F. D. Godman, F.R.S., and on Mr. J. G. Frazer, M.A., Fellow of Trinity.

On June 8, a number of foreign guests who had been present at the Stokes jubilee celebration and the Royal Institution centenary, were entertained at Oxford, and, in a convocation, the honorary degree of D.C.L. was conferred on Profs. Becquerel, Körner, Liebreich, Moissan, and Newcomb.

At a congregation at Cambridge on May 11, the degree of Doctor in Science (*honoris causa*), was conferred on Alexander Kowalevsky, the illustrious professor of zoology in the Imperial University of St. Petersburg.

On June 2 the University of Cambridge conferred honorary degrees on Professors Cornu, Darboux (Paris), Kohlrausch (Berlin), Michelson (Chicago), Mittag-Leffler (Stockholm), Quincke (Heidelberg), and Voigt (Göttingen).

Mr. Prillieux has been nominated member of the Academy of Science, Paris, in the botanical section, in place of the late Ch. Naudin.

The St. Petersburg Geographical Society has awarded its great gold medal to Dr. G. Radde, Director of the Caucasian Museum at Tiflis.

Mr. Alexander Agassiz has been elected president of the American Academy of Art and Sciences.

The gold medal of the Linnaean Society has been this year awarded to Mr. J. G. Baker, the well-known botanist of Kew.

The following naturalists have been elected foreign members of the Linnaean Society :—Adrien Franchet (Paris), E. C. Hansen (Copenhagen), Seiitsiro Ikeno (Tokyo), E. von Martens (Berlin), and G. O. Sars (Christiania).

It has been resolved to establish a professorship of Agriculture at Cambridge, subject to the following regulations:—The professor shall teach and illustrate the principles of Agriculture, apply himself to the advancement of the knowledge of the subject, and undertake the direction of the Department of Agriculture in connection with the University. The Professorship shall exist for ten years, and longer should the University so decide, and it shall not be tenable with any other Professorship or Readership in the University. The stipend shall be £800 per annum, or £600 per annum should the Professor hold a Fellowship. The Professor shall be connected with the Special Board of Studies for Biology and Geology, and shall be a member, *ex officio*, of the Special Board of Physics and Chemistry, and of the Board of Agricultural Studies.

Convocation at Oxford on May 16 passed a decree authorising the University chest to receive for the next five years £400 per annum from the Royal Geographical Society, and to pay the same to the common university fund, and also to pay that fund during the same period £100 per annum from the chest, the sums so paid to be applied to the furtherance of geographical study in Oxford. A provisional scheme for the teaching and study of geography has already been arranged.

The appeal made some time ago by the Duke of Devonshire, as Chancellor of the University of Cambridge, for financial assistance to the university, is meeting with substantial support, the list published showing promises which amount to over £50,000.

A statue of Charles Darwin by Mr. Hope Pinker, which has been presented to Oxford University by Mr. Edward B. Poulton, M.A., Fellow of Jesus College, Hope Professor of Zoology, was inaugurated at the University Museum, and an address was delivered by Sir Joseph D. Hooker, K.C.S.I., F.R.S., Hon. D.C.L. The Vice-Chancellor (the President of Corpus) presided, and among those present were Professor Charles Darwin of Cambridge, Sir

John Conray, Sir J. S. Burdon Sanderson, and Professor Poulton. The Chancellor, in opening the proceedings, said Darwin's method and Darwin's conceptions were applicable to the whole range of knowledge, and had been extended to numerous fields of research which probably, at the beginning of his speculations, never entered within his own purview. The historical method which had been so fertile in its results was indeed known and practised before the time of Darwin, but it was mainly owing to Darwin's splendid applications and illustrations of it in the natural sciences that it had now become the acknowledged and generally received instrument of inquiry in the sciences of mind, morals, aesthetics, language, society, politics, law, religion, and in fact every subject connected with the constitution of history and the capacities of man. The statue, which was pronounced as a remarkable likeness of Mr. Darwin, was unveiled amidst loud cheers.

The Johnson Memorial Prize of the University of Oxford has been awarded to Mr. H. N. Dickson of New College, for his work on the distribution of water and currents in the North Sea.

Women's munificence to universities and colleges in the past has generally taken the form of bequests, but Aberdeen recently received a handsome gift during a lady's lifetime. Miss Cruickshank, daughter of Dr. John Cruickshank, Professor of Mathematics in Marischal College from 1817 to 1860, gave not long ago £15,000 to establish a botanical garden in the city for the use of university students and the general public. The garden will be about five acres in extent, and situated in Old Aberdeen. It is intended to perpetuate the memory of Mr. Alexander Cruickshank, LL.D., brother of the donor, who was devoted to scientific pursuits, especially botany and geology, and who died about two years ago. The laying out of the garden is now in rapid progress under Prof. Trail's supervision. There will also be a physiological laboratory and other important adjuncts.

The North London Natural History Society sends us its programme for the latter half of this year. There are excursions to Broxbourne, Tring, Eynsford, Lambourn, Epping Forest, Kew Gardens, and "South Kensington Museum," as well as cycle runs. The papers offered seem to be, for the most part, of a general nature. Meetings are held at the Sigdon Road Board School, Dalston Lane, close to Hackney Down Station, and begin at 7.45 p.m. Those who wish to become members should apply to the Secretary, Mr. L. B. Prout, F.E.S., 246 Richmond Road, Dalston, N.E.

From the *Times* of June 15 we learn that Sir Harry Johnston devotes a section of his new report on Tunis to an account of the measures taken there for educating the native population. In the course of this he gives a very interesting account of the "Mosque of the Olive Tree" (Jama-Ez-Zituna) at Tunis, one of the three great centres of Mahommedan learning in North Africa, the others being El Azbar in Cairo and the Great Mosque at Fez, in Morocco. This Zituna still remains a great centre of teaching. It is an immense building with 161 porphyry columns, lit only by many open doors. Outside the main building is a vast square, surrounded by a colonnade, at one end of which is an immense minaret. Within the main building, where the porphyry columns are, is the sacred shrine, and in this main building the professors teach and the students learn. The institution has a valuable library of Arab books and manuscripts, some of which are said to have come from the famous library of Alexandria destroyed by the first Mahommedan invader of Egypt. Over 400 students are usually taught at this university, while there are about 100 professors. The lectures begin at sunrise and continue until sunset, 15 different lectures usually going on at the same time. Each professor sits cross-legged, with his back against one of the many columns of the mosque, his students grouped about him. The latter vary in age from 16 to 30, but occasionally are men of advanced middle age. They can choose their own professors, but are constrained to some

extent as to the course of teaching it is considered best for them to follow. They live near the mosque in *medressahs*, or lodgings, of which there are 22, each presided over by a sheikh or elder. The instruction is chiefly in theology, rhetoric, logic, grammar, law, and medicine, and much obsolete and useless teaching is given under these heads. Until recently there was but little method in the instruction; each professor rambled on in his discourse, ranging over any topic on which he cared to impart information, and the students listened or not as they chose. To encourage a more practical education, the State offered the students exemption from military service and from certain taxes if they passed an elementary outside examination; but only 4 of 66 recently succeeded in doing this. In future it is intended to impress on the management of the mosque that each professor should keep to one subject; that the students should be obliged to take notes and pass periodical examinations. Outside lectures on scientific subjects and on matters of present-day interest have also been established, and about 100 students from the mosque attend these.

The foundation-stone of a Museum of Oceanography was laid at Monaco on April 25. It will house the collections of the *Princess Alice*, and will include laboratories.

The salary of an assistant in zoology at the New York State Museum is $900, about £187 : 10s. This sounds promising. It is a pity that the notice of the last examination was not issued in time for the out-of-work zoologists in this country to send in their names.

The collection of shells of the late Mr. Henry D. Van Nostrand, recently given to Columbia University, is, says *Science*, well known among malacologists as one of the most valuable of private collections in the country; it contains the larger and better portion of the land shells of the West Indies collected by Thomas Bland, including many types, together with many of the rarest specimens of the Perry Expedition.

The Ballestier collection of shells from the East Indies made at the beginning of this century, has been presented by the heirs of Warren Delano to Harvard University, which has also obtained Mr. E. Elsworth Call's collection of American land shells.

The Gray Herbarium of Harvard University has, says the *American Naturalist*, recently purchased a collection of Compositae of the late Dr. F. W. Klapp, of Hamburg. It contains about 11,000 specimens, and will probably add 60 genera, 1500 species, to the Gray Herbarium, which previously contained 35,000 sheets of composites.

Dr. Daniel G. Brinton, professor of American Archaeology and Linguistics at the University of Pennsylvania, has presented to the University his collection of books and manuscripts relating to the aboriginal languages of North and South America. According to *Science*, the collection represents a work of accumulation of twenty-five years, and embraces about 2000 volumes, in addition to nearly 200 volumes of bound and indexed pamphlets bearing on the ethnology of the American Indians. Many of the manuscripts are unique. A number of the printed volumes are rare or unique and of considerable bibliographical importance. The collection of works on the hieroglyphic writings of the natives of this country embraces nearly every publication on the subject. The special feature of the library is that it covers the whole American field—North, Central, and South—and was formed for the special purpose of comparative study.

The new building erected in the Dublin Zoological Gardens in memory of the late Professor Samuel Haughton was formally opened on May 19 by the Lord-Lieutenant, in the presence of a large gathering.

The *Booth Free Library Museum and Technical School Journal* shows that every effort is made by Mr. J. J. Ogle to widen the influence of these institutions.

Popular lectures on birds have been delivered in the Museum, illustrated by specimens in the cases and books brought in from the Library; and the notes are now printed in the *Journal*.

This year no appropriation has been made for the New York State weather service. The sum is only $4500 dollars per annum, but with the volunteer aid of nearly 2500 persons, it has been enough to maintain a weather signal station in conjunction with the Bureau at Washington, to publish weekly "Crop Bulletins," much appreciated by the farmers, and to carry on observations, and numerous stations, some of which have continuous records for thirty years. "This interruption," says *Science*, "will make a break in the files which can never be repaired."

In its fifth session, which will be held in Germany in 1901, the International Congress of Zoology will award for the third time the prize founded by His Majesty the Tzar Nicolas II. The following subjects are proposed, though the whole need not be dealt with:—"Influence of light on the development of colours in Lepidoptera: the causes determining the differences of colours, form, and structure of parts covered during the resting position in insects."

The memoirs presented may be in manuscript or printed; in the latter case their date of publication must be subsequent to September 1898. They should be written in French, and addressed before the 1st of May 1901 to Prof. A. Milne-Edwards, 57 Rue Cuvier, Paris, or to Prof. R. Blanchard, 226 Boulevard Saint Germain, Paris. According to rule, naturalists belonging to the country in which the Congress is to be held are not eligible.

The 69th meeting of the British Association will commence, on September 13, at Dover, under the presidency of Professor Sir Michael Foster, who will deliver an address at 8 P.M. At two evening meetings, which will begin at 8.30 P.M., discourses will be delivered on September 15 by Professor Charles Richet, and on September 18 by Professor J. A. Fleming. The concluding meeting will be held on Wednesday, September 20, at 2.30 P.M., when the association will be adjourned to its next place of meeting.

The following are the titles of the sections and the names of the members who have been nominated by the Council for the office of President of Sections:—(A.) Mathematical and Physical Science, Prof. J. H. Poynting; (B.) Chemistry, Mr. Horace T. Brown; (C.) Geology, Sir Archibald Geikie; (D.) Zoology, Mr. Adam Sedgwick; (E.) Geography, Sir John Murray; (F.) Economic Science and Statistics, Mr. Henry Higgs; (G.) Mechanical Science, Sir W. H. White; (H.) Anthropology, Mr. C. H. Read; (I.) Physiology, Dr. J. N. Langley; (K.) Botany, Sir George King.

The meeting will have the special feature of being of an International character, as an interchange of visits has been arranged with the French Association for the Advancement of Science, which will hold its meeting this year at Boulogne. The members of the French Association will visit Dover on Saturday, September 16; and it is proposed that a formal reception of the visitors shall take place in the morning before the proceedings of the Sections begin, which they are invited to attend. The members of the British Association are invited to visit Boulogne on the following Thursday.

The Mayors and Corporations of Dover and Canterbury, the Military Authorities of the South-Eastern District, and the leading Scientific and Educational Institutions have signified their desire to take part in the entertainment of the Association.

The Castle, Docks, and National Harbour Works will be open for inspection during the meeting. Excursions will be arranged to places of interest in the neighbourhood of Dover, and there will be special Geological excursions in the afternoons. Excursions will also be arranged to Calais and Ostend, and a longer one to towns of Northern France and Belgium at the conclusion of the meeting.

The Reception Rooms will be at Dover College, in the old building of the Priory, close to the Priory Station (L.C.D. Railway), and within a few minutes' walk of the Sectional meetings, most of which are arranged to take place in the Municipal Technical Schools and adjoining buildings.

From the unique character of the meeting and the historical importance of the town in which it is held, a large attendance is expected.

At the annual meeting of the Royal Geographical Society Sir Clement Markham reviewed the geographical work of the past twelve months. In the course of his summary, Sir Clement touched on most parts of the earth's surface, and paid in passing a compliment to Major Marchand, the scientific results of whose journey across Africa could not, he said, fail to be very important. Sir Clement was able to give numerous hitherto unpublished details as to the progress of the *Southern Cross* expedition, of which Mr. Borchgrevinck is in command, but his main references to the Antarctic referred to the national expedition which is being organised under the joint auspices of the Royal and the Royal Geographical Societies. With obvious gratification he also referred to the establishment of a geographical school at the University of Oxford as "crowning the edifice of the Society's educational policy." The President was also able to announce the completion of a task of great magnitude and importance, in which the Society's librarian, Dr. Hugh Robert Mill, has been engaged for some years past—a complete geographical catalogue. This catalogue is a practically exhaustive list of the literature of every part of the earth's surface. It contains at present 100,850 cards, and is, of course, only available in the library of the Society, but it is to be hoped that it may at no very distant date be printed, and so made available for students generally. Another work of great utility to which Sir Clement referred was the preparation of an authoritative list of geographical terms, with definitions. To effect this, a special Nomenclature Committee has been appointed, and when its work is completed many persons besides professional geographers will have reason to be grateful. Unfortunately, neither of the gold medallists of this year could attend personally, Mr. Foureau being far away in the heart of Africa, and Captain Binger too much occupied with his duties at the French Colonial Office to come to London; so the medals were received on their behalf by the military attaché of the French Embassy. Another medal was presented—the gold medal of the American Geographical Society—which the American Ambassador handed to Sir John Murray, in recognition of his many brilliant services to geographical science.

The International Hydrographic and Biological Congress, which is to discuss the arrangement of periodical researches into the conditions of the North Sea and North Atlantic, was opened at Stockholm on June 16.

The Société Helvétique des Sciences Naturelles will meet at Neuchâtel from July 31 to August 2. A due proportion of discourses and excursions are intimated.

At the Geographical Congress at Berlin, this summer, the languages to be used will be limited to English, French, German, and Italian. The *Scientific American* notes a protest in the review published by the Madrid Geographical Society against the exclusion of the Spanish language, in view of the fact that it was spoken by most of the discoverers and colonists of a large part of the world. The writer says, if more geographers were able to read Spanish they would not from time to time bring forth facts as new which were printed in Spanish books two or three centuries ago.

The thirtieth volume of the *Report and Transactions of the Cardiff Naturalist's Society* for 1897-98, published 1899, as is so lamentably common in such cases (though in this case the delay is said to be accidental), has not been sent to us, which seems to us a mistake on the Society's part. It affords

evidence of the flourishing condition of the Society, which has 460 members, and it chronicles a creditable amount of appropriate work. We observe that the Society enlivens its autonomic functions by inviting experts from outside to give public lectures, and in this they seem to have proved their wisdom practically as well as theoretically, for they made a profit of about £125 on one lecture.

At the annual congress of the South-Eastern Union of Scientific Societies held at Rochester at the end of May, Mr. W. Whitaker, the President, gave an address on the "Deep-seated Geology of the Rochester District," and there were papers by Mr. Benjamin Harrison on plateau implements; Mr. J. J. Walker on collecting Coleoptera; Mr. G. F. Chambers on eclipses; Prof. G. S. Boulger on botanical bibliography and records; Mr. J. Hepworth on the history of the *Rochester Naturalist;* Mr. Paul Mathews on ideals of natural history societies; Mr. C. Bird on the position of science in education; Mr. E. Connold on vegetable galls. Prof. Howes was elected president of the 1900 Congress to be held at Brighton.

A striking result of the "Valdivia" expedition, in regard to which one naturally wishes to have more details, is (as translated in *Nature* from Dr. Supan's summary in the April number of *Petermann's Mittheilungen*) that "the quantity of plankton (in Antarctic waters) increases down to about 2000 metres, diminishing rapidly at greater depths, although no level is destitute of animal life. The quantity of vegetable plankton, on the other hand, reaches its lowest within 300 or 400 metres of the surface. The characteristic of the Antarctic plankton is the abundance of diatoms, and the occurrence of special forms; the appearance of the Antarctic type begins as far north as 40° S., but in 50° S. the presence of forms belonging to warmer seas is still noticeable."

Science for May 26 contains an account of ethnological work on the island of Saghalin by Dr. Berthold Laufer of the Morris K. Jesup North Pacific Expedition. There are certain differences between the Ainu of this country and those of Yezzo; their numeral systems is decimal not vigesimal, their dialect is more archaic, and its phonetics richer. Dr. Laufer has obtained explanations of many of their decorative designs, and much information as to traditions. Measurements were difficult to take, but the hairy nature, at least of Saghalin Ainu, is not so great as supposed. From the Olcha Tungus Dr. Laufer obtained wooden idols and amulets of fish-skin. Among the Gilyak he saw many secret ceremonies, and he induced both Gilyak and Tungus to sing into his phonograph. Altogether an excellent record of work, with suggestions of some excitement, danger, and hardship.

Dr. Zwingle, representing the Department of Agriculture of the United States, is now in Morocco on a mission which may open a new industry in the most arid sections of the South-west. It has been found that date-palms, with some irrigation, will grow as well in Arizona as in Arabia. Dr. Zwingle is making a study of the African date-palm, selecting the varieties best adapted to the American arid region.

Mr. C. A. Harrison, Jr., Mr. W. H. Furness, and Dr. H. M. Hiller, who recently returned from an exploration of Borneo, with collections for the University of Pennsylvania, are, we learn from *Science*, about to start on another expedition. They expect to make explorations in the northern part of Burma and make archaeological and ethnological collections.

Professor Gustave Gilson, of Louvain University, Belgium, has begun, under the direction of the Government of Belgium, a series of experiments in the North Sea resembling the observations conducted by Mr. Garstang from Plymouth. On April 29 a set of bottles was let off from the West Hindar light vessel, 2° 26′ E., 51° 23′ N., *i.e.* about 20 miles north-west of Ostend.

Each bottle contains a printed card, and it is hoped that any one who picks up one of these bottles will take out the card and fill up the blanks reserved for the place and date of finding, name and place if found on the shore, latitude and longitude if found on the sea, and send it to Professor Gilson.

A preliminary report upon the results of the scientific expedition to the island of Socotra has been issued by Mr. Henry O. Forbes, Director of Museums to the Liverpool Corporation, who, under the auspices of the Royal and Royal Geographical Societies of London, and of the British Association, and in conjunction with Mr. W. R. Ogilvie Grant, representing the British Museum, undertook the investigation of the natural history of the island. The expedition occupied a period of about six months, and the investigations were conducted amid considerable difficulties. At one time all the members of the party were laid down by a pernicious form of malaria, and they also suffered from frequent attacks of fever. The party were fortunate in discovering many new species of plants and animals, and a valuable collection has been brought home. According to the report the Socotrians are only poorly civilised Mahommedans, living in caves or rude cyclopean huts, and possessing but few utensils, implements, or ornaments, and no weapons. The ethnographical collection is consequently small. The plant specimens have been handed to a well-known student of the flora of Socotra, Professor I. Bayley Balfour of Edinburgh University, who describes them as of high scientific interest, and of great commercial value. The cultivation of some is being undertaken in the Royal Botanic Garden at Edinburgh. The report concludes by congratulating Liverpool on being the first provincial Corporation to further the advancement and increase of knowledge by actively sharing in the investigation of unknown regions.

The Indian Marine Service steamer, the *Investigator*, has recently closed a season of surveying, with important results both for navigation and zoology. The *Investigator*, starting from the Moulmein river in Burma last January, steadily surveyed—and her Surgeon-Naturalist, Captain Anderson, trawled—across the bay to the northern end of the great Andaman, and fixed the position of the island for the first time. Thence the longitudinal position of Port Blair, the capital of the penal settlement of the Government of India, was fixed by running a meridian distance to Double Island, off Burma. When at work in the Middle Straits between the two largest islands, the ship's staff had the assistance of forty tamed Andamanese pigmies against their as yet savage countrymen, who of late have killed several of the Indian convicts near Port Blair with poisoned arrows. The fifteen islands in the three groups of the Cocos, four Andamans and nine Nicobars, will henceforth be a help instead of a danger to the busy mercantile marine plying between Calcutta, Madras, Burma, and the Straits Settlements. The deep-sea trawl went down in some cases from 480 to 800 fathoms, from which Dr. Anderson brought up not a few valuable additions to his collections.

It is reported that the Duke of Abruzzi, the nephew of King Humbert, has started for Franz Josef Land, intending to penetrate as far as possible by ship, and then to make a rush for the Pole with sleighs.

Early in May a party of scientific men started for Alaska as the guests of Mr. Edward H. Harriman, of New York. Among those taking part in the expedition are Prof. Prichard, of the United States Coast Survey; Prof. Coville, of the Department of Agriculture; Prof. C. Hart Merriam, of the Smithsonian Institution; and Prof. William Trelease, of the Missouri Botanical Gardens. The American Museum of Natural History is represented by Frank Chapman and John Rowley, the Field Columbian Museum by Daniel G. Elliott, Amherst College by Prof. Emerson, Leland Stanford University by Prof. Gilbert. Messrs. R. Swain Gifford and Louis Agassiz Fuertes will go with the expedition as artists.

Mr. H. J. Mackinder, reader in geography at Oxford, has gone in charge of an expedition to explore Mount Kenia, in British East Africa.

We are glad to notice that the Technical Instruction Committee of the Liverpool City Council has been enlightened enough to set a good example, in arranging with Prof. W. A. Herdman to give a short course of lectures and demonstrations to help teachers in schools towards imparting sound instruction in natural science.

A discovery of coal, to which much importance is attached by geologists as bearing upon the coal seams pierced in Kent, is announced. The boring is situated a few miles south-east of Calais, and is one of several which have been put down, under the direction of Mr. Breton, the French geologist. The seam struck is two feet six inches thick, and is pronounced to be equal to the best quality of Welsh steam coal.

The *Scientific American* notes Dr. Koeppe's contention that distilled water is decidedly deleterious to protoplasm, absorbing from the same saline constituents and swelling its tissue even to the extent of destroying the vitality of the cells. Distilled water has a similar action on the cells of the stomach, producing in some cases vomiting and catarrhal troubles. He concludes that the toxic property of certain glacier and spring water is due to its absolute purity, which also explains why the sucking of ice and drinking of glacier water sometimes causes stomach derangement.

Dr. D. Hansemann has reported on the brain and skull of von Helmholtz. The head was about equal to Bismarck's, the brain was about 100 grams heavier than the average, the sulci were very deep and well marked especially in the frontal convolutions. Like Cuvier, Helmholtz was somewhat hydrocephalous in youth; and it has been suggested by competent authorities that this state, by enlarging the skull and allowing the brain more room to grow, may be rather an advantage than otherwise.

Natural Science

A Monthly Review of Scientific Progress

AUGUST 1899

NOTES AND COMMENTS.

Against the Tide.

A CRANK has been defined as a man whose position is so different from our own that we utterly fail to understand it. But this definition is too charitable; it ignores the public aspect of the crank, who not only occupies an unintelligible position, but bores you by insisting upon it. The crank is essentially a house-top man, not one in a corner. Yet we would not call any one a crank, for by the definition this would proclaim our own lack of understanding. We would only say that there are some whom some would call cranks, and we have just received a paper from one,—a paper entitled "Fausseté de l'idée évolutionniste appliquée au système planétaire ou aux espèces organiques" (Lyon, 1899, 7 pp). The author, Mr. F. Leport, has previously tried to convince geologists that there are no faults around Morvan, to convince astronomers that the nebular hypothesis is gratuitous, and to convince others about other things, and now he tries to convince us of the falseness of the evolution-idea. What he has convinced us of is, of course, that he does not understand it at all. He opposes it to the idea of creation, which no sensible man ever does, for to do so is to quarrel about punctuation. He finds that the law of existence is undulatory movement, and that the origin of the movement is divine—a platitudinarian belief which affects the evolutionist not one whit. He tells us about the homogeneity of protoplasm and the infertility of hybrids (surely we might have been spared *that*), and so with other matters, when he gets near facts he shows by mishandling them that he does not realise their solemnity. He tells us that a thesis of St. George Mivart's entitled "Evolutionisme restreint aux corps organiques" was examined at Rome by competent authority and judged "insoutenable" so far as it dealt with the body of man, and his lament is that the verdict was so limited in its disapprobation—"signe terrible des temps troublés où nous vivons." We would borrow from the Roman authority the word "insoutenable," and fix it to Mr. Leport's mistaken attempt to talk wisely about matters which he shows no evidence of understanding.

A Rare Rotifer.

In October 1859 Professor Semper discovered in some ditches intersecting rice-fields in the Philippine Islands a remarkable spherical rotifer, which he named *Trochosphaera aequatorialis*, in allusion to the ciliary wreath which divides it into two hemispheres. For thirty years nothing more was seen of the creature, until Surgeon Gunson Thorpe found it (1889) in Fern Island pond of the Botanical Gardens at Brisbane. In 1892 he discovered in some irrigation creeks and ponds near Wuhu on the Yangtsze Kiang a new species (*T. solstitialis*) in which the ciliary wreath encircles the body as the Tropic of Cancer does the earth. In 1896 the same species was found in the Illinois River by Dr. C. A. Kofoid, and in 1898 by Mr. H. S. Jennings, in a pond close to Lake Erie. We have taken this information from a short note by Mr. C. F. Rousselet (*Journ. Quekett Micr. Club*, vii. (1899) pp. 190-193, 1 fig.) who recently exhibited to the Quekett Club a slide of *T. solstitialis*, prepared according to his method by Mr. Jennings. This was the first time the animal had been seen in the flesh in England. "The anatomy is extremely simple and beautifully displayed, all the organs, usually so indistinct and closely packed together in rotifers, being here spread out and suspended in the transparent sphere in the most delightful manner." It is said that Dr. Kofoid is preparing a full account of this remarkable type.

Does the Organism Repeat Itself?

In an interesting paper entitled "Localised Stages in Development in Plants and Animals" (*Mem. Boston Soc. Nat. Hist.* v. (1899) pp. 89-153, 10 pls.), Mr. Robert Tracey Jackson elaborates an interpretation which is in direct line with the ideas expressed by Hyatt, Cope, Ryder, Beecher, and some other American workers. It is especially in harmony with Hyatt's law of senile characters:—"In the old age, stages are found which are similar to stages found in the young, and are prophetic of types to be found in degradational series of the group to which the animal belongs." But Mr. Jackson's particular point is that in addition to stages in the young and in the old age, stages may be found in localised parts throughout the life of the organism.

"In organisms that grow by a serial repetition of parts, it is found that there is often an ontogenesis of such parts which is more or less closely parallel to the ontogenesis of the organism as a whole. In the ontogeny of such localised parts in a mature individual we find stages in development during the growth of the said parts which repeat the characters seen in a similar part in the young individual."

Such localised stages have been observed in the leaves of plants, in branches or suckers of plants, in the budding of some of the lower

animals such as *Hydra* and *Galaxea*, in the plates of crinoids and sea-urchins, in external ornamentations in molluscs, and in the septa of cephalopods. They must be clearly distinguished from stages in the development of the organism as a whole, for they are features seen in localised parts throughout the whole life, or are capable of being brought into existence by certain conditions throughout the life.

The author adduces a large number of illustrations from plants and animals, and sums up: "The occurrence of localised stages, and their bearing, may be expressed in the following law, which should be compared with the laws concerning youthful and senile stages:—Throughout the life of the individual, stages may be found in localised parts which are similar to stages found in the young, and the equivalents of which are to be sought in the adults of ancestral groups. While this law covers the usual conditions, it is possible and even probable that degradational or progressive features may appear as localised stages. To include such cases the following clause may be added: The equivalents of regressive or progressive localised stages are to be sought in the adults of degradational or progressive series of the group."

Mr. Jackson's thesis is an attractive one whose applicability must be tested in detail and with impartiality, and it will be interesting, therefore, to see how experts on foliage and budding, fossil sea-urchins and cephalopods deal with it. That it is a luminous suggestion carefully illustrated and tested, and not a mere bow drawn at a venture, is something to be thankful for.

Nephrite.

In the *Globus*, vol. lxxv. No. 18 (May 6, 1899), attention is called by A. B. Meyer to some fresh occurrences of nephrite in Styria. In 1883 he found pebbles or rolled fragments of it in the river-beds of the Sann, near Cilli, and the Mur in Gratz. That these pieces of nephrite were really pebbles was, in both instances, questioned, some considering them to be stone implements which had been more or less water-worn and rounded.

In 1888 Berwerth also found nephrite in the bed of the Mur, and in the present year discovered three more examples among the rolled fragments of that river, one of them measuring 3·6 metres.

These later finds are considered by Berwerth to remove all doubt concerning the occurrence of nephrite in Styria, and to indicate that it will probably be met with forming thin beds in the metamorphic rocks in the vicinity of the river Mur, an opinion in which Meyer, from his earlier observations, perfectly concurs.

The Ordeal by Fire.

A YEAR or two ago, Drs. Hocken and Colquhoun of Dunedin witnessed the fire-walking ceremony in Fiji, and their scientific zeal led them to lick the soles of the feet of the natives who were about to walk over the red-hot stones to ascertain whether any substance had been applied to them. Colonel Gudgeon, British resident at Rarotonga, has now gone one better and walked over the stones himself, and appears to have enjoyed it.

In the March number of the *Journal of the Polynesian Society*, published in Wellington, N.Z., he says that the tohunga, or priest, first took across Mr. Goodwin, at whose place the ceremony was performed. He then said to Mr. Goodwin, "I hand my mana (power) over to you; lead your friends across." Mr. Goodwin then led Colonel Gudgeon and two other Europeans across. Colonel Gudgeon got across unscathed, and only one of the party was badly burned. They all walked with bare feet, and after they had done so, about two hundred Maoris followed. Colonel Gudgeon did not walk quickly across the oven—which was about 12 feet in diameter—but with deliberation, for he feared that he might tread on a sharp point of the stones and fall, as his feet were very tender. His impression as he crossed the oven was that the skin would all peel off his feet, but all he felt when the task was accomplished was a tingling sensation, not unlike slight electric shocks, on the soles of his feet, and this continued for seven hours or more. Many of the Maoris thought that they were burned, but they were not, at anyrate not severely. Although the stones were hot enough an hour afterwards to burn up green branches, the skin of Colonel Gudgeon's feet was not even hardened by the fire.

We should like to know the experience of Dr. Craig, who was badly burned. Was he one of the percentage who are said to be non-susceptible to suggestion? Or is the solution elsewhere?

American Species of Peripatus.

THE suggestive value of the systematic study of the species of *Peripatus* is well known. The isolated position of the type, its archaic and synthetic characters, its wide distribution, its great diversity of structure within narrow limits, the differences in the modes of development in the several species, and other considerations, lend special interest to the detailed working out of the taxonomy. The student of species is here almost forced to face the problem of origins.

In a recent communication on the American species (*Comptes Rendus Acad. Sci. Paris*, cxxviii. 1899, pp. 1344-1346) Mr. E. L. Bouvier notices some results of general interest. He mentions the

occurrence of *Peripatus* in some localities not previously recorded—Mexico, Guadeloupe, and Antigua. He notes that the American forms agree in having lingual teeth formed by a chitinous cone whose internal cavity opens by an apical orifice, in showing a clear dorsal median line usually attenuated to microscopic dimensions, and also a clear (probably sensory) organ on each side of the clear dorsal line in each of the grooves of the body. These organs are absent or atrophied in the African species (except *P. tholloni*) and in those of Oceania. But of greater interest is the note that the American species form small regional groups, more or less distinct, so that it may almost be predicted that each island of the Antilles has its particular species or variety.

Wearing of the Green.

One always welcomes a paper—however short—from Prof. Dr. August Gruber, so well known for his investigations on the Protozoa. One of his latest contributions (*Ber. Naturf. Ges. Freiburg*, xi. 1899, pp. 59-61) describes the prosperity of a colony of green amoebae which he observed for about seven years. The colony hailed from a water-basin in the Connecticut valley, and came to Europe in some dried bog-moss in a letter from Prof. Wilder. The green amoebae fed at first on what they could get in the vessel in which the bog-moss was placed; they devoured rotifers and various forms of rhizopods; but soon they and green specimens of *Paramaecium bursaria* were left in possession of the field of pure Freiburg water. No conjugation was observed, and, still more strange, no division, though crops of small forms appeared in continuous succession. The condition of prosperity was obviously to be found in the chlorophyll of the zoochlorellae in the amoebae, and in the sustained illumination. Samples placed in darkness soon came to an end. Thus Dr. Gruber has shown that organisms which are in ordinary circumstances holozoic may by the wearing of the green prosper for many years in a holophytic existence.

Brevis esse laboro, obscurus fio.

Writers of scientific papers, of text-books, and of museum-labels are ever too apt to judge of other people's knowledge by their own. Now one may be no fool and yet be absolutely ignorant of many matters that the specialist has at his fingers' ends. An author therefore should do himself the justice to remember that his papers may possibly be referred to by the general zoologist, or by the "remote, unfriended, solitary" (and shall we add?) occasionally "slow" student, and he

should write accordingly. There is a certain tendency to brevity, born either of natural slothfulness or of a more laudable thrift, but in all cases to be kept under restraint. This tendency is very noticeable when an author begins to quote from others. Nowadays mere shame prevents one from omitting the bibliographic reference altogether; but, oh! how easy it is to keep it short and to render it just so unintelligible that the reader will never bother to verify it! With what apparent sincerity, what underlying artfulness, we allude to "a ridiculous statement by M. Chose (C.R. CIX. '87, p. 20)" or to "the great discovery by A. M'Grabham (P.R.S.E., V. p. 251)"! These cabalistic letters are in themselves enough to give an air of supreme authority to our estimate. A few such references constitute an impregnable line of fortifications.

A further instance of the obscurity begotten of brevity is furnished by that peculiar convention which forbids the zoologist and botanist to write a fellow-worker's name in full when quoting him as authority for a generic or specific name. To write "De Candolle" instead of "DC," "Linnæus" instead of "L." or "Danielssen and Koren" instead of "D. & K." would stigmatise one's work as that of a mere beginner, unworthy of serious consideration. Naturally the constant repetition of the same name or names many times on every page of a systematic work would be intolerable, and if it really be absolutely necessary to quote the authority for every specific name each time it is used, then some fairly intelligible abbreviation is forced upon one. We, however, have often expressed our opinion that such repetition is an idle absurdity. But, just in those cases where the citation of an author's name would be useful, there the customary abbreviation is apt to deprive it of any value. The visitor to a museum sees a label "Wood of *Abies nobilis* Ldl."; the reader of a natural history book finds under a figure "Shell of *Voluta nivosa* Lam." What, beyond mere bewilderment, can these symbols convey to his mind? And in these places brevity is not needed, for there is nearly always plenty of space to spare in a label or a legend. Here are some contractions taken at random from a text-book of zoology; we should like to know how many professed zoologists, to say nothing of university students, can say straight off what they mean:—M. & W., W. & M., Fbs., Tric., Stp., Mas. & Alc., Wr., M. & T., Gm., M. V. K. To attempt to regularise these contractions, as the Germans have done, by the publication of a list of authors' names, is only to emphasize the evil. A new edition of such a list would be needed each year, and even if it were rigidly adhered to by systematists, one could not expect every field-botanist or every lover of birds to keep a copy perpetually at his elbow. No! let us give up this attempt to put natural science on a par with the missing word competition. Do what we may, the *Annals and Magazine of Natural History* will never attain the popularity of *Answers* or *Pearson's Weekly*.

The preceding remarks were prompted by a paper entitled "A Hunt for a Name," contributed by T. S. Hall to the *Victorian Naturalist* for May, 1899. The difficulties to which we have alluded are of course magnified in outlying parts of the world, where fellow-workers are few. In trying to name a coral, Mr. Hall found himself referred by the reporter of the *Challenger* to "*Plesiastraea urvillei*, Milne-Edwards and Haine, Cor. II., p. 490." On this "almost meaningless reference" Mr. Hall remarks: "When one knows the country it is easy for him to find his way about, but to the stranger it is not easy, and he needs the finger-posts which the other never heeds. 'Cor. II.' is good enough for the specialist, but is a meaningless 'blaze' for the 'new chum.'" We are glad that Mr. Hall refused to regard "Cor. II." as a Biblical reference, and that he eventually discovered "Histoire Naturelle des Coralliaires"; but what language would he have used had the *Challenger* reporter followed the custom of his kind, and contented himself with "*P. urvillei*, E. & H., Cor. II."?

The Parietal Eye.

THE parietal eye and adjacent organs of the New Zealand Tuatera (*Sphenodon*) form the subject of an important paper by Mr. A. Dendy in the May issue of the *Quart. Journ. Micr. Soc.* It has already been shown that in the adult of this reptile this eye is better preserved than in other animals; and the author now demonstrates that its development has undergone less modification than in other reptiles. The first indication of its appearance is seen at a stage (*K*) comparable with a two-day-old chick, when a "primary parietal vesicle" buds on the roof of the fore-brain slightly to the left of the median line. At stage *N* the eye forms a hollow vesicle in front and slightly to the left of its so-called "stalk"—the "parietal stalk," which is a finger-shaped diverticulum of the root of the fore-brain, practically in the middle line. The eye is almost or completely separated from the stalk, which contains a prolongation of the cavity of the brain. The "paraphysis" likewise makes its appearance at this stage, as a backwardly-directed outgrowth of the roof of the fore-brain.

At stage *O* the parietal eye and stalk are conspicuous externally; while at stage *R* (the one immediately before hatching) the eye, which is now apparently median, is seen as a white spot with a black border, the latter representing the pigmented margin of the retina and the former the lens. In the adult (stage *S*) the eye, though very highly organised, is no longer recognisable externally; but in recently hatched individuals it is stated to be still visible as a dark spot through the translucent skin covering the parietal foramen.

After discussing the structure of the eye and its nerve, and the

relations of the former to the stalk, the author states that the evidence in favour of the originally paired character of the parietal eye is derived principally from the fact that it arises to the left of the median line, while the stalk is practically median, and therefore slightly to the right of the eye. Accordingly the parietal eye in *Sphenodon* is regarded as the left of the original pair, while the right one is represented by the parietal stalk. It is shown that the origin of the latter appears to be precisely similar to that of the former; and the two have also a very similar structure, although the stalk never acquires the same degree of perfection as the eye.

The relations between the parietal stalk, the "epiphysis," and the brain are next discussed, not only in *Sphenodon*, but in Lizards, Cyclostomes, and Fishes. It is shown that in the two reptilian groups the epiphysis, or pineal gland, is a composite structure, in which the paraphysis takes a large share, whereas the parts comparable to the epiphysial outgrowths of Fishes form but a small one. In Lizards the stalk may represent either the right or the left parietal eye. Beyond that of fellowship, the parietal eye has no real connection with the parietal stalk, being supplied with a special nerve of its own quite distinct from the stalk. Finally, it is inferred that the ancestors of existing Vertebrates were furnished with a pair of parietal eyes, which may have been serially homologous with the existing functional pair of ordinary eyes.

The Expansion of the Empire of Ribbed Toads.

A SINGLE ribbed toad has been found at Humptulips, Washington, U.S.A. This simple statement involves a noteworthy fact. The sub-order of tailless batrachians, known as Costata, embracing the single family Discoglossidae, to which the new genus belongs, "has been credited with a most extraordinary geographic distribution. Until now it was composed of four genera, three of which are confined to the south-western corner of the palaearctic region, except a single species at the south-eastern end of the same region. The fourth genus, composed of a single species, represents, alone, the batrachia in New Zealand. None of the seven species known to form this sub-order consequently had been found in the Western Hemisphere at all, and none has thus far been taken in tropical Africa, Australia, or Asia, with the above exception. The addition of a typical costate genus to the fauna of North America is therefore not only an interesting novelty in itself, but it emphasises the fact that we have as yet much to learn about the geographical distribution of the vertebrates even in regions which have been fairly well explored." Thus writes Mr. Leonhard Stejneger, who describes the specimen in *Proceedings of the U. S. National Museum* (xxi. pp. 899-901, pl. lxxxix. June 1899).

The genus is called *Ascaphus*, meaning "spadeless," apparently because the sternum appears as a narrow band of cartilage only, without posteriorly diverging lateral styles as in other genera. But since the sternum "had been considerably damaged by the collector cutting open the abdomen to admit alcohol to the intestines," its shape is "a little doubtful," and may possibly not justify the generic name. An undoubted criterion is afforded by the position of the vomerine teeth, which are between the choanae, and not, as in other genera, behind them. The species is called *A. truei*, because Dr. True is the author's official chief. The sex of the unique specimen is not stated.

Degrees of Protective Adaptation.

An examination of the contents of the stomach has often proved of value in biological research, though it may seem to some a dull way of getting at the secrets of life. We have learned, for instance, not a little in regard to the habits of fishes through the patient labours of those who have analysed the contents of fishes' stomachs; and a recent research by Mr. Sylvester D. Judd (*Amer. Naturalist*, xxxiii. 1899, pp. 461-484), who has examined the stomachs of fifteen thousand birds, is an important contribution towards solving one of the most intricate problems of biology—the efficiency of protective adaptations.

These protective adaptations in insects are, as every one knows, extraordinarily diverse, but the most important are included under the following heads:—resemblance to surroundings; hairs; stings or poisonous bites; ill-flavoured, ill-scented, or irritating properties; warning coloration; and protective mimicry. These are the headings used by Mr. Judd in his paper, the broad result of which shows that the supposed protections of insects are certainly not always baffling to birds. He gives a long list of so-called protected forms, and of the birds which nevertheless prey upon them.

We agree entirely with the author when he says: "It seems to me that there are different degrees of protective adaptations—that some are much more effective than others. There is need of some standard of the efficiency of protective adaptations, *i.e.* a measure of their working forces. Some of the writers on the subject have led one to suppose that a good many protective devices secure almost complete immunity from the attacks of birds; while other investigators have been tempted, when they found in particular instances that facts, apparently, did not coincide with current views, to abandon the theory entirely."

There is an anthropomorphism in biology which is hardly to be got rid of. Because an insect is unpalatable to us we argue that it must be distasteful to a bird; but "it does not follow," Mr. Judd

says, "that since a stink-bug nauseates our stomach and irritates our tongue, it will produce a like effect on a crow." There appears to be need of a little more avian psychology, as he quaintly phrases it. "Numerous species of bugs and beetles which, in addition to being protectively coloured, possess ill-smelling, bad-tasting, and irritating secretions, would naturally be supposed by some writers to be avoided generally by nearly all birds, but they are habitually eaten by many birds of the eastern United States."

The conclusion seems to be, as we have said before in these columns, that adaptations are by no means so perfect as is often supposed. Protective adaptations may lessen the chances of death, and thus be of much evolutionary importance without being in any wise perfect. But it is fairer to let the author sum up:—"The alleged protective coloration is not the all-important factor in securing an insect from extermination, as some earlier naturalists have supposed; there are other equally important factors that demand consideration."

An Entomological Exhibition.

PROFESSOR BOUVIER, of the Muséum d'histoire naturelle of Paris, announces that a great entomological exhibition is being arranged for in the laboratory of his department, and asks for co-operation. The preliminary prospectus, given in *La Feuille des Jeunes Naturalistes*, July 1899, is very attractive, and includes the following divisions:—Bees and apiculture; giant arthropods and giant nests; wasps' nests; classification and anatomy, with especial reference to flight and stridulation; reproduction and development; adaptations—defensive, such as mimicry and protective coloration—offensive, such as weapons—and in relation to change of habitat; commensals and parasites; social insects; bizarre forms; domestic forms; useful and injurious insects, and so on. It is a big undertaking, which well deserves the co-operation asked for. To see such an exhibition will be an entomological education in itself.

At Last?

A PAPER by Professor L. Errera, entitled "Hérédité d'un caractère acquis chez un champignon pluricellulaire" (*Bull. Acad. Roy. Belgique*, 1899, pp. 81-102), cannot but arouse the interest of evolutionists. Has the long-sought-for instance been found at last? Is there a modification in regard to which we can look the whole world in the face and say that it is transmitted? The story will be read with bated breath, as the advertisements of novels say.

Conidia of the mould *Aspergillus niger* were cultivated (A) in a Raulin solution, (B) in a Raulin solution plus 6 per cent of common salt for one generation, and (C) in the same for two generations.

Then they were placed in a Raulin solution plus 18·4 per cent of salt, in which A showed no germination, B slight germination, and C general germination; again, in a Raulin solution plus 6 per cent of salt, in which A produced spores in 5 days, B in 4 days, and C in $3\frac{3}{4}$ days; and again, in a Raulin solution without additional salt, in which A showed sporification in 4 days, B in 5 days, C in 5 days, but slight.

Spores from the last-named three cultures, in a normal Raulin solution, were then sowed in Raulin solution plus 18·4 per cent of salt, in which A′ showed after 5 days no germination; B′, after 5 days, just visible germination; and C′, after 5 days, clearly visible germination.

Hence, it is argued, that the conidia of *Aspergillus* become adapted to the medium in which their parent is growing, and more adapted after the second generation than after the first; and, as the adaptation to a concentrated medium is not wholly lost after rearing in a normal medium, there is evidently a persistence of the adaptation, an inheritance of the acquired quality of resistance to concentration.

In truth, however, this is not very convincing. The distinction between soma and germ-cells is not more than incipient in the mould in question; and even if it were more marked, what does the case show but that the germ-plasm may be affected *along with* the soma by a saturating influence, which nobody can deny.

We need more than this before we allege the inheritance of an acquired character. We wish to hear of a clear-cut somatic modification observed to occur in successive generations, and of the recurrence of this modification or of some change in the same direction in the offspring when these are reared in a environment from which the original cause or stimulus of the modification is absent. At the best, Errera's case is no more cogent than those which have been adduced from the study of alcoholism, where the germ-cells are apparently affected along with the body—cases with which Weismann has duly dealt.

We may, however, recall David Harum's words: "A reasonable amount of fleas is good for a dog—they keep him f'm broodin' on bein' a dog;" and re-interpret them, saying that a reasonable amount of such experiments as those of Errera is good for Weismannists—if so be they keep them from brooding on the perfection of their system.

Colours of Northern Monocotyledons.

Mr. JOHN H. LOVELL has arranged, according to their colours, the 1058 species of northern monocotyledonous flowers recognised in the "Illustrated Flora" of Britton and Brown, and finds there are 41 yellow, 82 white, 22 red, 22 purple, 34 blue, and 857 green or dull, the last set being of course enormously swollen because of the large number of grasses, sedges, and the like. It is useful to have the facts of colour-distribution clearly before us, and when we have this it is almost impossible to refrain from drawing inferences, which may or may not be correct. Those which Mr. Lovell has drawn (*Amer. Naturalist*, xxxiii. 1899, pp. 493-504) are the following:—

The primitive colour of the perianth of the monocotyledonous families was green, as it still is in the greater part of the species which are anemophilous or self-fertilised. A few of the oldest families, with an indefinite number of stamens and carpels spirally arranged, have probably never possessed floral envelopes.

Yellow, white, and lurid or greenish-purple flowers, have in numerous instances been derived directly from the primitive green; red flowers have passed through a yellow or white stage; and blue and purple-blue have been derived from yellow, white, or red forms. Reversion to white is most common, but reversion to red or yellow also occurs.

Physiological conditions appear to have often played an important part in determining the coloration of the petals, while "insects have contributed to the fixation of such characters when once acquired."

In general, among monocotyledons yellow flowers are visited by bees and flies; white flowers by bees, nocturnal lepidoptera, flies, and beetles; lurid-purple by flesh flies; red by bees and butterflies; and blue chiefly by bees. Red and blue flowers usually have the honey concealed, which is a far more effective cause of the limitation of insect visits than colour. When the honey is abundant and exposed, and the flower pleasantly odorous, it may prove attractive to any anthophilous insect.

The Proper and Improper View of Heredity.

WE are not aware of the specific diagnosis of the journal called *The New Age*, edited by S. C. Mukhopadhaya, M.A., and published in Calcutta, but we know that it has a larger circulation (guaranteed) than *Natural Science*, and we see very prominently on its title-page an advertisement of a firm of plumbers and gasfitters, to which, indeed—unless to its position above the title—we have no objection, for the

association of science and art is one of our dearest ideals. We are afraid, however, that our mineralogical colleagues might not like the make-up of this "journal of universal information," for in the number before us the 5th heading is mineralogy and the 6th is science. It was an announcement under the last heading that arrested our hungry eye—"The Proper and Improper View of Heredity"—for this went beyond our furthest ambitions. We had cherished an idea that, with the help of Galton and Weismann and their opponents, we might in the course of time arrive at a discrimination between the true and untrue view of heredity, but the criterion of propriety seemed unattainable. We wondered before we opened the pages what revelation might await us—an exposure of Pearson's prolegomena as prurient, of Weismann's wisdom as wanton—and our fancies flew to Zola and Ibsen and other students of heredity, as we speculated whose views *The New Age* regarded as "improper." The very title, we say, was a wonderment to us. We had never thought of looking at the facts in the light of propriety, and yet how luminous it is! But when we came to the article we found only a feeble protest against the old, absurd misunderstanding that to recognise one factor in life means a denial of the others. "Let us never fold our hands and say, because we have inherited a poor memory, a small order, poor calculation, or imperfect digestion and weak lungs, that we are fated by that inheritance and cannot overcome it." Thereafter followed some verses on "Heredity's Opposites"—*e.g.*, "Lowest sinner, highest saint, dull of wit and full of *plant*" (the italics are ours), ending with the appropriate words "curses deep."

Darwin's Doggedness.

In the charming address which the veteran botanist, Sir Joseph D. Hooker, delivered on June 14, when Mr. Hope Pinker's statue of Darwin, presented by Prof. Poulton, was unveiled at the University Museum at Oxford, there are many little touches which vivify the picture which modern naturalists have of their master. The proof-sheets of the *Beagle* journal impressed Hooker profoundly, even despairingly, "with the genius of the writer, the variety of his acquirements, the keenness of his powers of observation, and the lucidity of his descriptions." In 1844 Hooker was shown confidentially a sketch of "The Origin of Species," and on his many visits to town he was habitually "pumped" after breakfast with botanical questions, the answers to which were deposited in bags or pockets that hung against the wall. "If I were asked," he said, "what traits in Mr. Darwin's character appeared to me most remarkable during the many exercises of his intellect that I was privileged to bear witness to, they would be, first, his self-control and indomitable perseverance under bodily suffering,

then his ready grasp of difficult problems, and lastly, the power of turning to account the waste observations, failures, and even the blunders of his predecessors in whatever subject of inquiry." As is well known, Darwin was wont to attribute his success to industry rather than to ability. "It is dogged that does it" was an expression he often made use of. He attributed his results to "the love of science —unbounded patience in long reflecting over many subjects—industry in observing facts, and a fair share of invention as well as of common sense." This is a famously modest self-estimate, but its psychological justice may be doubted, and it seems to us important to notice Sir Joseph Hooker's opinion. "In this retrospect he has, if my judgment is correct, greatly undervalued invention, that is originality or that outcome of the exercise of the imagination which is so conspicuous in every experiment he made or controlled, or in the genesis of every new fact or idea that he first brought to light." Truly it was fell doggedness.

Dispersal of Seeds.

AMONG many interesting notes in Mr. Clement Reid's "Origin of the British Flora" is a table of modes of dispersal of seeds, which may be quoted as follows:—Minute seeds readily moved by accidents of all sorts; those eaten or dropped by birds, most of which are destroyed while some remain uninjured; seeds passed in an uninjured state by mammals or birds; those transported by wind; those which cling to feathers or fur (*e.g.* in the cakes of mud which adhere to the flanks of oxen); those transported by water; those plants of which broken pieces grow, such fragments being carried on the legs of wading birds often to great distances. With regard to the transportation by water an interesting observation has reached us from Mull and Iona. It is said that thousands of apple seeds have taken root on those islands, the result of dispersal from the wrecked liner "Labrador." Mr. Reid mentions an interesting case of a dead wood-pigeon found by him in a chalk pit; its crop was full of broad-beans, all of which were growing well, though under ordinary circumstances they would have been eventually digested. As he says—"A pigeon would easily cross the Strait of Dover in half an hour, and in the days when raptorial birds and wild cats were plentiful many pigeons must have been struck down with their last meal undigested."

Reformed Nomenclature!

PROF. HERRERA emphasizes the impossibility of recognising organisms by their names under the present complicated system of nomenclature

in botany and zoology. No one can profit by the 800,000 names recognised by naturalists. For who can tell from the name anything about the nature of *Mormops megalophylla*, *Sphaeria sobolifera*, etc., etc. One cannot even say whether one is dealing with plants or reptiles, with crustaceans or zoophytes!

It seems then worthy of consideration whether we should not in current usage suppress the generic name, and leave it for the lists and treatises of specialists, whether we should not in current usage substitute for the generic title some composite term indicative of the class and family to which the organism belongs.

Thus all the names of mammals might begin with the syllables *Mammi*, and end with abbreviations indicative of the family. Thus we would suggest *Mammicanae lupus*, *Mammivespertae megalophylla*, *Mammileporae* or *Mammileporus cuniculus*.

If there are two equivalent specific names in the same family, one might add the complete generic name in brackets.

He goes on to suggest—

Avigallinae domesticus.
Reptilacertiae ocellata.
Piscipercidae fluviatilis.
Molluskhelicae aspersa.
Legumpapilliae sativus.
Insecticarabae auratus.
Echiniholothuriae regalis.
Arachniacariae scabiei.

Such a procedure seems to him easy and logical. The radicals Mammi-, Avi-, Crypto-, Insecti-, recall the sulphates, carbonates, ethyls, etc., etc., of the chemists; and would not vary in any important degree within a century. It seems the only way of securing a universal biological terminology, and besides saving an infinitude of time, it would conform to the mode of the exacter science of chemistry. Such is Mr. Herrera's suggestion. It should make the sticklers for terminology 'sit up.'

Science and Conduct.

Those who, taking an interest not only in science but in human conduct, desire to harmonise their conceptions of the one and the other, should not fail to study Prof. Münsterberg's recent volume on "Psychology and Life."[1] It is not light reading. As the author says in his Preface—"I do not want to entertain by these papers, I want to fight; to fight against dangers which I see in our public life and our education, in art and science; and only those who intend serious and

[1] Archibald Constable and Company. Pp. xiv. + 286. Price 6s. net.

consistent thought ought to take up this unamusing book." But it has all the charm of boldness, originality, and evident conviction. Whether we agree or not we are forced to think. There are, too, many passages which stimulate by their piquancy. Of the greatest possible happiness of the greatest possible number, "that discouraging phrase in which the whole vulgarity of a naturalistic century seems condensed," he asks, "is it really the source of inspiration for an ideal soul, and does our conscience really look out for titillation in connection with a majority vote?" Again in the essay on "Psychology and Mysticism" he says: "The telepathists annihilate the theosophists, and the spiritualists belittle the telepathists; and when the Christian scientists and metaphysical healers on the one side, the mind curers and faith curers on the other side, have spoken of each other, there remain few abusive words at the disposal of us outsiders."

The gist of Prof. Münsterberg's argument, so far as it can be presented in a few words, is as follows. Physical science deals with the phenomena of which it treats in terms of matter and motion; mental science devotes its attention to states of consciousness. The one leads to materialism, the other to idealism. Both are right within the limits of an ideal construction elaborated for specific ends. Both are utterly wrong if they seek to impose their special *isms* beyond these limits. In other words their final conclusion is scientifically valid but philosophically monstrous. Human life and conduct present abundant material both to physics and to psychology, material to be explained in terms of cause and effect; but "the interests of life have not to do with causes and effects, but with purposes and means; in life we feel ourselves as units and as free agents, bound by culture and not only by nature, factors in a system of history and not only atoms in a mechanism." This may seem to some a hard saying; nor will it sound less hard when it is urged that the real world of purposes and teleological ends in which we live is endlessly fuller and richer than that shadow of reality which we mean by physical and psychological existence. There are plenty of hard sayings in Prof. Münsterberg's book. But though we may not agree with some of his main positions which appear to us open to criticism, he knows quite well what he is discussing, he is trained alike in physics and psychology, he is well acquainted with the stock, and often cheap, arguments of the materialist, and he is a thinker whose thought is not to be lightly disregarded and brushed aside simply because it does not chance to be consonant with our own. Hence we commend his book to serious naturalists, who can spare some attention to human affairs, not necessarily for acceptance but at any rate for careful consideration.

ORIGINAL COMMUNICATIONS.

Some Considerations Concerning Symmetry.

By Professor R. J. Anderson.

Symmetry has so much to do with the order, form, and arrangement of parts in natural objects and figures geometric, that one becomes interested in its varieties, the causes of these latter, and the relationships that exist between them. There is involved also the question of asymmetry. Symmetry is the outward and visible sign of the resultant forces that fashion a body. There is no limit to the number of forms that may be assumed, but with certain kinds of symmetry one becomes more familiar than with others. Bilateral symmetry is one of these. Corresponding to a part on one side of a bilaterally symmetrical body there is a part on the other side, the parts thus appearing to balance one another like weights in scales. A three-legged table, or other utensil of a tripod nature, seems to suggest more completeness because of the greater steadiness. The four-limbed symmetry of the vertebrate, and the six, eight, ten or more legged insects, spiders, crabs, etc., are instances of the bilateral. Radial symmetry is to be observed in numerous organisms, *e.g.* many plants, sea anemones, and star-fish, and is commonly distinguished from the bilateral.

The sphere is the most generally symmetrical solid body. It is divided into two parts by any plane passing through its centre. The spheroid is divided into two symmetrical halves by every plane passing through its axis of rotation, and by the equatorial plane. The general ellipsoid can only be divided symmetrically by three planes. The right circular cylinder can be divided into two similar parts by any plane passing through the axis. The right elliptical cylinder can be divided into two equal halves by two planes only, passing through the axis, and the right circular and elliptic cones conform to this rule. If the cylinders and cones be oblique only one plane can divide those solids symmetrically. These are only special forms of the infinite number of possible cones and cylinders. The conceptions and practical investigation of complex figures gradually become impossible to all except

a few, and at last even to these. Yet even a superficial study of such figures and forms must lead one on to the consideration of the forces at work. There is exhibited on approaching the living form a remarkable feature which living things possess beyond inorganic forms, viz. the greater power and facilities which a living organism has to express what it cannot conceive or understand, and the capacity of adjusting most complex forces to meet others which it can neither measure nor weigh.

The forces that are at work in moulding bodies are external or internal; amongst the latter may be placed surface tension in fluids. The external compression that causes a soft substance to assume a spherical form is more familiar to us than the mode of action of the cohesion forces that cause the particles to swing into position to form the crystalline body. Yet one may in inorganic bodies see that the forces that press, or the pressure that acts all around a sphere, may be so distributed as to form a cube, if divided into three equal pressure sets, each two forces acting opposite to one another on equal areas and at right angles to the directions of the other two pairs. The cube, octohedron, or dodecahedron (with rhombic base), may be easily produced by similar compressions, and these symmetrical irregular bodies may be divided into two equal symmetrical parts by three planes or more passing through certain axes. It is evident that a quadrilateral symmetry may be noted in a cube lying on one side, by making sections with suitable planes, and a triangular symmetry in sections made perpendicular to a through diagonal. A suitable adjustment of the compressing forces leads to the production of the square prism. The side pairs of pressure sets will in this case be equal, whilst the end pair is greater or less, but each pressure pair acts at right angles to each of the other pressure pairs. The lateral compressing forces, if one opposing pair do not act at right angles to the other opposing pair, will give rise to a rhombic prism. The three main axes must stand at right angles. If the compression be so applied that an oblique prism is produced, one plane only can be found which will divide the crystal into symmetrical halves. Where a crystal is doubly oblique, the form may be imitated by proper pressure planes, no plane of symmetry can be found; symmetry here is only discoverable in individual planes. The hexagonal prism form seen in beryl and other minerals is connected with the rhombohedron, and the rhombohedron is a cube crushed out of shape. The tetrahedron and pentagonal dodecahedron are asymmetric crystalline forms, although regular solids.

Angular bodies are not limited, as is well known, to inorganic nature. The elements of which organic bodies are composed are often constrained to assume forms with an angular outline. Polyhedra, hexagonal prisms, tessellated pavements, brick shaped and stellate cells, are a few of the varieties well known to the student. These forms, although correctly attributed to external pressure, are largely under the influences of forces inorganic and organic within the elements themselves.

It is evident that a limit to the exercise of the compressing force may be set by the elasticity of the cell contents resisting any further compression, or extreme pressure may paralyse the cells. Then light, heat, and electric phenomena, as well as gravity, are agents that may influence the demeanour of the cells. The radiate symmetry of a hexagonal prism body or element is easy to understand, but the prism may be divided bilaterally by six planes that pass through the axis, and notably by three directed through the axis and opposite angles. Skeletal structures laid down along the lines of certain radii, where circumstances favour the deposit, establish the character of the symmetry, and these radial structures (composed of lignin, lime salts, cellulose, or other substances) leave between them avenues which protoplasm and fluids keep free. The skeleton, like many another tissue, is advantageously regarded as an excretion, such as might be cast off by some organisms, but is retained by its possessor. This structure, of seeming advantage at first as a protecting and supporting framework, grows so large sometimes as to interfere with the activity of the tissue by which it has been produced. There are apparently no limits to the possibilities in the interior structure of cylindrical organisms. The number of radii may be many or few, and the cylinder may be of small or large diameter.

The trimerous and pentamerous symmetry of plants excited much interest when first established as a plant law. The fixity and nature of growth of the higher plants favour a radiate cylindrical symmetry.[1] There are well-known cases of an apparent bilateral symmetry, in the ovary and other parts, and a spurious quadrilateral in others. The increase in information with reference to the effects of light, heat, gravity, etc., forces most people to be cautious in drawing conclusions. Dr. William Allman, formerly Professor of Botany in the University of Dublin, sought to connect the structure of exogens with the pentamerous arrangement of the parts of the flower, and that of the so-called endogens with the trimerous arrangement, by means of the cellular structure of the plants. Starting with the hypothesis that plant-cells in mass have a tendency under the influence of an all round pressure to assume figures intermediate between the sphere and regular solid, he refers to the fact that the regular solids are: the tetrahedron (4 sides), cube (6 sides), octohedron (8 sides), dodecahedron, with pentagonal faces (12 sides), icosahedron (20 sides). He proceeds to show that the two latter forms appear to agree best with the forms of cells in plants, the dodecahedrons would best explain the pentagonal arrangement of the exogens, and the icosahedrons the trimerous form of endogens. The cubical form was regarded as more prevalent amongst the acotyledons. Allman supposed the young shoot of a

[1] The term symmetrical is used sometimes by authors when bilaterally symmetrical is meant. The word is also used to indicate certain relationships between sepals, petals, stamens, etc.

plant to consist of columns of dodecahedral cells, arranged so that the upper surface of one cell might coincide with the base of the one next above it. If the adjacent columns fit as nearly as possible into one another, that is to say, that the re-entrant angles of one column may correspond to the salient angles of the other, three dodecahedra will meet at each edge, but, since the angle of a dodecahedron is less than 120°, they will not fill the space, but will leave interstices, increasing in width from the centre of the mass towards its circumference. The "tubes" will find room to grow in these interstices, and the growth will be effected by the addition of matter externally as in exogens. The increase is likely to be more considerable where the edges meet, that is, at the angles of the pentagon, than elsewhere. Certain qualifications are, however, introduced. If the cells are icosahedral and arranged in the same manner, it is easy to see that, their angles being greater than 120°, the interstices would be formed internally, and that the growth of such a plant would proceed by the internal addition of matter as in so-called endogens. In this case, as in the exogens, the growth should take place along planes passing through the angular points. Hence the parts ought to be arranged in threes in the one case and in fives in the first. The parts in the fructifying organs of certain fungi and mosses are in number powers of two, so, it is pointed out, that the cubical arrangement in acotyledons is rendered probable.[1] This ingenious hypothesis ("Une idée au moins piquante et ingénieuse," says De Candolle) was propounded in the earlier years of the present century. The elements, although angular, unite to form tissues with round outlines. The form assumed is the result of various forces. Equally diffused pressure acting along the radii of a cylinder tends to maintain its form. A cone would have its shape best maintained by the diffusion of the pressure according to a certain law; but here again the internal activities, surface tension of cells, perhaps, and other agents, may materially modify the results.

One cannot venture to compare the increase in size of a crystal to the deposit of a soluble salt from an evaporating solution, but rather to the growth of a battalion of soldiers by more men falling into rank all round at the word of command. Even in crystals many are the causes that affect the increase in size and form; temperature and impurities in the substance are two of the best known. The "growth" here is, of course, influenced by the supply of material. Organic bodies, also, are influenced by many activities that start from without and reach into their substance. Their growth is true growth, but within considerable limits the physical demeanour of the organic may correspond to the inorganic.

One might compare a slender shoot to a six-rayed ice crystal that is growing slowly by the addition of an upward stream of water.

[1] Abbreviated from Allman's paper. The term "tubes" appears to have been used to indicate vessels and fibres of plants as distinguished from cells proper.

The flow of nutrient fluid in the plant conjoins with the active protoplasm to make new tissue. Year after year new additions are added to the stem, but these are laid down in accordance with the laws of plant growth. Whatever may be the resolution of these forces, it is evident that the form, shape, and nature of the grouping of bundles, and the succession, as well as the shape of the conjoined bundles and packing tissue that form stems or leaves, are the results of not merely internal forces, physical and organic, but external forces of great constancy, if not of great magnitude.

A collapsing cylinder is said to assume often the form of a three-sided prism, and a sphere the form of a tetrahedron. There can be no harm in placing side by side with this statement the record of trimerous symmetry in plants. One would require to take a note of several hollow cylinders in the latter case, perhaps, which renders the comparison more difficult; five, six, or eight-angled prisms might also be allowed to be within the powers of plant manufacture,—columns not to be formed as a battalion of soldiers, from the outside alone, but by the addition of new rows between the already formed lines. W. Allman pointed out a connection between the icosahedron and dodecahedron; if the latter be inscribed in a sphere, tangent planes at the angles will constitute an icosahedron, just as a cube in a sphere similarly treated will give rise to an octohedron, and a tetrahedron to a figure like itself. It may be noted here, that, if we compare the pentamerous symmetry with the trimerous, it will appear at once that five equilateral triangles[1] meeting by their apices and arranged so that each is separated from his neighbour by twelve degrees, will leave chinks which in triangular prisms would serve for young tissues. Account is rather taken here of the collective tissue groups (vascular and cellular). The flower or leaf parts, if followed to the large stems, are not so easy to marshal. Six equilateral triangles meeting in the centre by their apices, and lying in the same plane, would leave no spaces for the reception of cells or fibres; in this case the exterior of the composite bundle might be regarded as the chief generating tissue. Then eight equal equilateral triangles with the apices turned in would require to stand well out in the same plane in order that their external angles might even fit to one another. Eight equal equilateral triangular prisms may be adjusted, with their long axes parallel to one another, and with their edges on radial planes that divide the cylinder into equal segments. One face pointing out in each, and one edge looking in, will, if the prisms stand, leave interspaces internally wide and externally narrow. These prisms, if the first to develop out, might determine the course of future tissues. The arrangement of the leaves on the stem suggests other schemes for plant bundles, but there is clear enough proof of a predominant radial symmetry, and it does

[1] The triangles are here taken to represent sections of prisms. No account is taken of any twisting the stem or bundles may experience in the course of development.

seem odd that the two forms of prisms that the trimerous and pentamerous symmetries suggest are asymmetric. The fact that arboraceous monocotyledons dwell in the tropics, and that dicotyledons dwell in temperate regions, has been commented on. The Dicksonias of New Zealand and the araucarias of South America have chosen curious places for homes. The sun in rotating on its axis, in sending its rays through an atmosphere that partly polarizes the rays which are going through the air with various degrees of obliquity, and the same luminary in having its countenance affected by spots occasionally, not to speak of the various wave whirls that may affect rays going in different directions, may be held responsible for some of these discrepancies. The rays, if they are of such a nature as to be alterable by a crystal, may be naturally expected to have some power to alter the character of a crystal, or other substance, and so a crystal may get a molecular twist, and the plant that uses the crystal as food may become similarly influenced, or get directly altered itself; but although there may have been a tendency to molecular twisting in the young plant by the sun's rays, grown plants are not so apparently affected; the plant tissues seem to have some power of correction, and so the difference in the effects of the symmetry of the rays in the north as compared with the southern hemisphere is not observed.

The symmetry of animals is of various kinds. The spherical kind is illustrated in the Protozoa. The Radiolaria, with their rays and their trellis work, show us what was, or is, being done, and raise inquiry as to the various agents that may be at work in bringing about the result. Still water or some inert fluid may be looked upon as favouring the maintenance of the spherical form seen in the resting stages of many Protozoa, but the surface tension may also contribute largely to the result. The sea anemones, simple sponges, and corals are admirable examples of the modified cylindric symmetry; the medusae illustrate the modified spherical symmetry. The mouth in the centre with appropriate radiating tubes, and in some cases the actual provision of separate segments with a definite nervous system, shows a very important departure in the bearings of the symmetry of a body on its life. The welfare of many an animal is so much connected with its colonial habits that its separation often means rapid extinction. The chance of extinction is diminished by the segmentation in question. Each part is, in a manner, independent of its neighbours; so are the parts of a star-fish, which may live after separation. A single ray may even turn over. A mechanical advantage seems also to be derived from a pair of fixed planes placed at right angles to one another, both as regards purchase and security, in the case of certain medusoids. The rhythm of *Rhizostoma* seems independent of the symmetry, 20 to 24 contractions per minute in a closed vessel were noted in one case. The rhythm is best counted in the sea, however, an operation which is only possible there in some medusae. The motion of the fluid from centre

to circumference may in part be responsible for the radiate character of the tubes, but the other forces already alluded to in other structures cannot be lost sight of in this connection, nor the fact that the contraction and dilatation of the umbrella favours the circulation of fluids in certain directions.

Passing over the tunicates, which may be radial in colonies and bilateral in individuals, the worms, arthropods, and vertebrates may be noted. A bilateral symmetry is here evident enough. Not only in the early forms, but in the adult life of many of these and molluscs, a disguised radiate symmetry seems to prevail.

The chief axis of the yolk sac in the chick may be regarded as an axis of symmetry in the young animal. There may or may not be the remains of an apparently azygous organ, but a radiating system of alimentary tubes is easy to see in some animals, and a like arrangement in the nervous and vascular systems in others that are easy to group with a central axis. The paired ganglia above and below the anterior part of the alimentary canal in worms and arthropods, and the three pairs of ganglia in the molluscs, may also be regarded as an exaggerated radiate symmetry. Then the alimentary canal has been looked upon as forming the central axis of the system, an axis often strengthened by lime or chitin, deposited or formed in a tissue derived from without; the cells also that form bone are probably derived from the outer embryonic cell layer. The vascular system consists chiefly of four tubes in some worms (dorsal, ventral, and lateral). The nervous system may occupy the sides in the central part of the body, or dorsal and ventral cords may be both present in the same animal. This bilateral symmetry might be regarded as a modified kind of quadrilateral symmetry. The special development of certain parts emphasizes the former variety. The dorsal tube feet in some holothurians are dummies, whilst in others are three rows of tube feet on the ventral surface, and two on the dorsal. There are indications of a bilateral symmetry in the interior. The enamel of the teeth is derived from a portion of the invaginated skin in the vertebrates; so, if, passing over the early stage, it be desirable to take the alimentary canal as the axis of symmetry, some ingenious attempts may be made to give force to the assumption. The position of the primitive mouth will not then escape attention, nor will the fact that the sympathetic has a good district in the alimentary canal. If this study be pushed as far as one can decently go, and the ground changed to the spinal canal and cord, then a most instructive method of comparison may be noted, viz. on the dorsum a canal, a nervous cord around it, and the appropriate serous membrane, blood vessels, muscle, and bone; and, on the ventral part, the intestinal canal, a sympathetic neuro-muscular system, serous membrane, vessels, etc.

Around the vertebrate axis a modified radial system seems to prevail. Owen and Humphry advocated this, although not in so

many words. Owen's typical vertebra, it will be remembered, has growths above, below, and at the sides. The two dorsal growths end in the spine; the lateral growths are the transverse processes (dorso-lateral), and the lower growths (ventro-lateral) may join the ribs which form an arch like the dorsal one. The limbs are represented by diverging appendages. The limb folds seem to partake of the quadrilateral symmetry type in some fishes. Humphry pointed out that the term "duality" is inapplicable to the nervous system and skeleton. The lineal axis of the embryo sends off the processes referred to, and there is therefore a quadrilateral rather than a bilateral symmetrical arrangement.[1] Humphry, however, distinguished between the body as a whole in this regard and the separate parts. Leaving out the bodies of the vertebrae which are variously formed, but originally developed round an endodermic growth, one can make out a radiate symmetry of four, five, or six rays, according as certain processes are counted or omitted. The pillars of the dorsal arch may be counted separately, so may the transverse processes and body processes; or, reckoning each pair as one process forming a two-pillared arch, there are four arches. The spinal nerve cord section occupies the dorsal arch, the sympathetic the ventral, and the posterior root ganglia are at the sides. It is clear, however, that the spinal cord may be looked upon as made up of two lateral halves, so may the sympathetic cords. A survey of the entire system tends to render the bilateral symmetry of each less clear, whether taken together or separately. The sympathetic seems to be of more considerable relative importance in early life, judging from the drawings of Paterson. The ganglia are often large in man, but the size appears to be due in the abdominal ganglia to fibrous tissue (D. J. Cunningham). W. Alexander has removed the superior cervical in man with advantage to the patient, proving how far the system has gone back.[2] The sympathetic is, however, of enormous interest because of its distribution, subsidized by the spinal, in the viscera and arterial coats. The symmetry that takes account of the spinal cord, divided into two equal lateral parts, has also reference to the division of the abdominal nervous system, so that a modified quadrilateral symmetry may appear as a bilateral symmetry. The dorsal and ventral systems, as every one knows, are mainly independent of one another. The presence of the serous membranes secures this independence in part, but the nerve connections do not favour a ready transference of impressions from one system to the other. The connections, however, come into use often in disease, and a slight activity in the terminals of either systems, may produce a profound disturbance in the district supplied by the other. The sympathetic ganglia associated with the cerebral system are obscured by the magnitude of the large brain and its connections in vertebrates. The

[1] See Quain's "Anatomy," 8th ed.

[2] Nerve cells being now proved to be trophic only, the fibres collectively assume more prominence in our estimate of the value of a nerve tract, or district.

significance of some of these ganglia has been satisfactorily learned. The sense organs bear out apparently the statement that vertebrates are, speaking generally, bilateral animals. The pineal eye, and the arrangement of the sense organs in some invertebrate types, may be cited as being favourable to other views of symmetry. It will be remembered that C. S. Minot thinks that the cerebral ganglia of a worm may fairly be regarded as the optic central organs, and that some of the suboesophageal would do for cerebral ganglia if the mouth were further back.

Asymmetry.

The five fingers and the five nerves that form the brachial plexus have been associated by some anatomists (Paterson), but Bardeleben has given reasons for regarding the primitive hand as having a much larger number than five digits. The Gasteropods show rare examples of asymmetry. The left respiratory organ and the left kidney in part lose their character, and the right organs do the work of the pair. Mechanical causes seem to be the main agents in bringing about the absorption of the absent organs. A superficial bilateral symmetry appears in some, but not only is there want of dorso-lateral symmetry, but the dorsal growth of the animal has been so considerable, and the form has become so altered dorsally and ventrally, that with the exception of a portion of the body in front, it is impossible to see an approach to quadrilateral symmetry. There are, however, the four ganglia or six, which may be looked upon as part of a radial quadrilateral or hexagonal symmetry. The renal organs of the lancelet are sometimes asymmetric. The newly-hatched sole is symmetric; the size is 3·55–3·75 mm. long. This creature swims with its yolk sac up because the latter is light (Cunningham). The eyes come to lie on the upper surface (the right). Remembering that if a fish is to forage and rest on the floor of a bay, it must be spread out laterally or have some supporting apparatus in connection with its fins, it seems natural that the sole or plaice, not being able to make suitable provision in either of these ways, should simply lie on its side and turn its second eye up. The result is advantageous in this way, that a surface of one to two square feet is presented to the view of a voracious dog-fish, skate, or shark, so that the apparent size may save the sole or plaice. The asymmetry is, therefore, susceptible of a triple explanation. The diminution of one lung in snakes is due to the elongation of the body; with the elongated lung a certain amount of dislocation of the viscera is associated. The single lung is, under the circumstances, better suited for respiration. The single ovary in birds is most convenient in consequence of the large eggs, and the large ovary is connected with the persistence of the abdominal rather than the chylopoietic aorta. The latter is, evidently, the best for mammals. Asymmetry in the dolphin tribe is marked in the skull. The large left upper canine

tooth of the narwhal emphasizes the condition. This asymmetry is not easy to explain. Is it due to the dolphin opposing one side by preference to an ocean current, so that he grows gradually one-sided, like a sensitive politician? or does he get altered by attempting to present a too bulky broadside to an opposing foe for the purpose of increasing his self-importance, or to reassure himself? May the change have been brought about as the result of deep nervous impressions received from without? This creature lives near the surface a good deal, and sees much that is one sided among the phenomena of aerial nature. The contemplative disposition may allow the reflex nerve actions too much range.

The well-known cases of asymmetry in man may be mentioned—the left aorta and heart, right sided liver, left stomach and spleen, large right lung, the lateral spinal curve. The viscera may be transposed in position. Asymmetry is found sometimes in the muscles of man. The chest region may display asymmetry, the sternum or ribs may be more prominent at one side. The pelvis also shows occasionally some features of asymmetry. The skull, in the size and thickness of the cranial bones, is subject to some variations. The bones on one side are sometimes thicker than those on the other side, as has been shown by the writer elsewhere; the sinuses of one side are sometimes larger than those of the other. The septum of the nose is often bent to one side. Bilateral symmetry in man seems to be the rule. Humphry laid much stress upon this fact, but he takes occasion to refer to the specimen of a skeleton of a boy in the Bonn Museum, in which the bones of the right arm and leg are longer than those of the left side. The disproportion was marked by nodules in the leg bones, but not in the arm bones. These nodules indicate the former presence of inflammatory action in the right lower extremity. The right humerus is 9 lines and the ulna 10 lines longer than those of the left side. The right femur is 11 lines, and the right tibia 2 inches longer than the left ones.

The nervous system has considerable influence over distant parts within certain limits. Asymmetry is thought by some to afford some indications of permanent central nervous change. Abundant statistics are necessary in order to come to any satisfactory conclusion. Lombroso found in one class (Class A) of offenders 26 per cent of cranial asymmetry; in Class B, 46 per cent; Class C, 32 per cent; Class D, 50 per cent. Asymmetric faces were found in 7·7 per cent of delinquents and in 1·8 per cent of another class. Criminals have the advantage (?) of others in possessing a larger percentage of wry noses (not due, presumably, to mechanical causes). Asymmetric faces are commoner in classes B and D than in other types. It is also stated that anomalies are more common in man, especially savage man, than in woman, and more common amongst males of other vertebrates than in females (Viazzi quoted by Lombroso). It will be remembered,

however, that anomalies are more common in man than in other mammals. In the latter anomalies of all kinds are rare. The production of a deformity, owing to some peculiar mental state, is not easy to follow out. There are very many factors at work. The mental and physical defects may be concomitant effects of the same cause, or the latter may be very remotely connected with the former. A deformity, if exposed, is, on the other hand, not necessarily associated with any aberrant mental condition. A structural change in the central nervous system may be associated with some distal change, but the distal change may be due to easily explained mechanical causes.

If we revert to asymmetry in crystals, it will be recollected that attempts have been made to explain their asymmetry in their action on light, by referring to the asymmetric character of solar radiation. Some crystals rotate the plane of polarization to the right, others to the left, and two opposites are compared to a pair of gloves. The sun's rays, passing south (as has been noted earlier in this paper), may be expected to produce effects on vegetable structures different from those produced by the north-going rays or the intermediate ones. The question of the effects of the sun-spots arises naturally. If these asymmetric rays and the portion of the solar surface exposed has favoured the growth of dicotyledons in one place, monocotyledons of great dimensions in another, and giant ferns in a third, what is to prevent our speculating on the changes that may have resulted from certain alterations in his demeanour in ancient times? Did the sun show less or more of one pole to the Silurian world? Was this followed by a bend that gave rise to the vegetable products of the carboniferous? Was another change attended with the growth of the Triassic, and another with the growth of the Jurassic flora, until at last, after a tropical and cold period, the present temperate vegetation of the north, and the palms in the tropics and Dicksonias in the south, have been evoked by some new position of the solar globe?

In special breeds of domestic fowl abundant material can be obtained and the history can be studied. The sternum is often marked by a crooked keel, and the tail-bone and feathers are sometimes wry. The bend of the keel is sometimes to the left and at other times to the right. A large number of specimens have been examined, but taking fifteen at random, there is a distinct bend to the left in nine keels and to the right in six. Tracing one of the best marked, the keel at the anterior part is seen to be a little bent to the right, followed back it leads to the left, crosses the middle line, forms a curve of considerable length, and, turning in to the median line, recrosses it to the right side.

Two-thirds of the breeders consulted by me are of opinion that crooked sternum keels are hereditary, and that in-and-in breeding is accountable for the wry tails.

One-third of the breeders consider the causes to be mainly

mechanical and due to the nature of the roosts. These breeders look upon the weakness naturally associated with the preparation of pure breeds of fowls as a predisposing cause.

Light pure bred fowls have been often observed to have crooked keels, whilst heavy breeds, if the birds are not allowed to roost early, have not the deformity. The following is a note from a breeder:—" A 'black Norfolk turkey' with a crooked breast was mated with a straight-breasted hen. All the chicks got the same treatment, the roosts were low and flat, and covered with straw until the birds were able to fly." Notwithstanding these precautions five cock birds out of the sixteen birds which formed the flock had crooked breast keels. Water-fowl have sometimes crooked breasts; the deformity here is not due to roosting. The most crooked sternum in my possession belonged to a Brahma. The keel, where the bend is greatest, is nearly horizontal. There are marks of pressure on the keel edge in some cases. A distinct broadening of the edge of the keel is perceptible, in two bent to the right, and in four bent to the left. An indentation occurs in front of the middle of the two keels bent to the left. Two keels have marks of having been broken and reunited. The wry tail has been attributed to the bird roosting too near the wall, and to the tendency to form a compensating bend in consequence of the breast being bent to the opposite side. The fanciers who believe that it is due to inherent weakness because of the breeds being run out, seek to correct the tendency by the introduction of new stock. The wry-tailed birds are discarded. The evidence goes to prove that—

(1) Malformation is commonest in pure breeds.
(2) In-and-in breeding tends to develop wry tails and crooked keels.
(3) The distortion is frequently transmitted from parent to offspring.
(4) Roosting on round or sharp roosts tends to promote the distortion.

Summary.

(1) The shape of a body may be due to forces within or pressure without, or both.
(2) The same kinds of symmetry are to be observed in inorganic and organic forms.
(3) The forces at work inside organisms are "vital" and physical. The resultant figures are the expression of the work of two or more sets of agents.
(4) Asymmetry may be due to causes internal or external, or both.

I have to thank Dr. G. J. Allman for the opportunity of consulting his father's manuscript.

QUEEN'S COLLEGE,
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The Flora of the Alps.

By Professor Alfred W. Bennett, M.A., B.Sc., V.P.R.M.S.

Even to those tourists who claim no botanical knowledge, the pleasure of a visit to Switzerland is greatly increased by the extraordinary beauty and variety of its flora. Even in the lowland valleys and on the spurs of the foot-hills, the wild plants, if not more varied and more beautiful than our own, present many novelties, at least to the dwellers in our southern counties. In the early spring the meadows are gay with the globe-flower and the bird's-eye primrose; later on the monks-hoods, yellow and blue, the hellebores, the anemones, the phyteumas, the pinks, the gentians, the yellow foxgloves, have the charm of novelty; and the keenest delight is experienced when the blue bells of the Soldanella are first seen peering through the snow, or the Edelweiss is first gathered in its rocky home. It is only the experienced botanist who realises that, as a compensation, some of our most beautiful wild flowers are absent from the flora of Switzerland. We can well under-stand the rapture with which the great Swedish botanist Linnaeus is said to have gazed for the first time on a gorse-common in full bloom; for the gorse is not abundant in Central or Northern Europe. Our bell-heathers hardly go east of the Rhine, and may be said to be replaced on the Swiss mountains by the "alpine roses" or rhodo-dendrons. The wood-hyacinth and the purple foxglove are not found in Switzerland.

The distribution of the alpine flora in Switzerland is very unequal. The calcareous Jura has a subalpine flora of its own. The flora of Mont Blanc and of the Alps of Savoy is a very poor one. That of the Bernese Oberland is somewhat richer. But the great wealth of the alpine flora is south of the Rhone valley; and especially in those mountain spurs and alpine valleys which stretch into the territory which is geographically and linguistically, though not politically, Italian. The Rhone valley itself exhibits a remarkable commingling of different floras. Here I have gathered, almost side by side, the subalpine holly-fern (*Polystichum lonchitis*) and the gigantic horsetail (*Equisetum ramosissimum*) representative of the Mediterranean flora.

With regard to the special characteristics of the flora of the Alps,

the most familiar and most striking is the abundance of the flowers, growing either in great masses or remarkable for their large size and brightness of colouring. This is exhibited in various ways. In the first place, we may compare the alpine with the lowland species of genera which are represented in both floras—for example, *Aquilegia alpina* with our columbine; *Dianthus alpinus* or *glacialis* with our pinks; *Scutellaria alpina* with our skull-cap; *Bartsia alpina* with our British species; *Myosotis alpestris* with our forget-me-nots; the Edelweiss with our cudweeds; and many others that might be mentioned. Or we may take genera that are exclusively or chiefly alpine, as far as the European flora is concerned:—*Gentiana, Primula, Pedicularis, Rhododendron, Soldanella, Saxifraga, Sempervivum,* etc. These are among the most familiar glories of the alpine flora. Or, again, we may take genera common to high and low altitudes, but in which the alpine species are characterised by the small flowers being so crowded together as to make the masses of them very conspicuous from a distance, such as *Arabis, Silene, Moehringia, Draba,* and many others.

The advantage to alpine plants of the conspicuousness of the flower is obvious. Although not so dependent as lowland plants on the production of seeds for the perpetuation of the species—the great majority of them being perennials—yet, like many of our own perennial plants, trees and others, they do, as a rule, produce abundance of ripe seeds, and for the carriage of pollen from the anthers to the stigmas they are largely dependent on the visits of insects. Now, at great altitudes winged insects are comparatively scarce, and it is obvious that a conspicuous and far-seen sign as to the locality where they can find their honey must greatly increase the number of flower-visits which they can pay in the course of a sunny afternoon. Mr. G. W. Bulman has recently, in the pages of this journal,[1] ventured the opinion that four of the keenest-sighted naturalists who have ever studied the phenomena of plant physiology—Darwin, Wallace, Lubbock, and Hermann Müller—are all mistaken in their interpretation of the function of colour in flowers, and that insects are attracted to flowers mainly by the sense of smell rather than by the sense of sight. My own observations, which have extended over many years, lead me to range myself unhesitatingly on the side of those distinguished names. That insects are, to a certain extent, attracted by the odour of flowers is undoubted. But in the Alps this can only come into play to a very subordinate extent. Very few alpine plants are strongly scented; and, if they were, owing to the strong winds that almost constantly prevail at those great heights, the scent would be almost useless in indicating its source to insects. In the bright colour and large size or close crowding of the flowers, we have, on the other hand, an obvious and admirable adaptation to this end.

But it does not by any means follow that the sole purpose of the

[1] *Natural Science*, Feb. 1899.

bright colour of flowers is to attract insects. We find it in flowering plants where it can have no such function, as in the scarlet stigmas of the hazel, which is unquestionably anemophilous, and in the young inflorescence of the larch; or, in Cryptogams, more especially in connection with the organs of reproduction, as in the brightly-coloured oogones and antherids of *Chara* and the red sporanges of *Sphagnum*. There can be little doubt that the bright red colour has an important function in absorbing and retaining the heat-rays, and thus maintaining the organ at a temperature necessary for the physiological processes going on within it. Hence the very earliest of the flowers of the Alps, like Soldanellas and Hepaticas, are usually very brightly coloured, and the earliest spring foliage has also very commonly a more or less bright red tint.

There are other and equally interesting characteristics of alpine plants. And here it may be worth while to contrast the conditions of life in high altitudes and in high latitudes, which are often assumed to be very similar. They are, in truth, totally different. In the arctic or subarctic zone we have a brief summer, during which there is almost perpetual insolation and a nearly uniform temperature throughout the twenty-four hours; in Switzerland the summer nights are longer than they are with us, and the difference of temperature between day and night is often excessive, the nights being associated, even in the height of summer, with exceptionally heavy dews. It will be seen, therefore, that we have totally different climatic conditions to deal with. We have in our own flora several arctic species which do not occur in Switzerland, as, for example, *Saxifraga nivalis* and *Primula scotica*.

Alpine plants have several other characteristics besides the large size or close crowding of the flowers. In the first place, although many ripen abundance of seed, but a very small proportion, as has already been mentioned, are annual. In many the floral organs are almost completely formed within the flower-bud during the preceding autumn, so that they are ready to unfold with the first warm days of spring, and before the appearance of the leaves, not requiring these organs to supply them with any further food-material. Hence the very early flowering of many alpine and sub-alpine plants, such as the hepatica, Christmas rose, winter aconite, species of *Soldanella*, *Primula*, *Gentiana*, etc. Secondly, from the great strain to which they are subject from violent winds, we find a considerable number with prostrate woody stems, species of willow, birch, etc., such as we seldom meet with in plants of our own climate. For the same reason the root-system is also often very strongly developed in comparison with the aerial part of the plant. Furthermore, the extreme brightness of the sun during the summer months has a tendency to cause excessive transpiration or evaporation from the leaves, which has to be counteracted by specialities of structure. This protection is afforded

in many ways. In some the leaves are thick and fleshy, as in species of *Sempervivum*, *Pinguicula*, etc.; or they are crowded together in dense rosettes, as in so many members of the orders Cruciferae or Caryophyllaceae. Others are covered with a dense felt of hairs, as in species of *Achillea*, *Artemisia*, or *Gnaphalium*, including the Edelweiss. In others again protection is afforded by the rolling back of the margin of the leaf, as in *Azalea procumbens*, *Empetrum nigrum*, etc. The greater rarity of the air at high altitudes implies, of course, a smaller supply of carbonic acid gas from which to build up the food-materials of the plant. Hence the organs in which alone this manufacture of food-materials can take place, the green leaves, are almost invariably strongly developed.

In a very interesting series of experiments carried on by Prof. G. Bonnier in his experiment-station at Fontainebleau,[1] he appears to have established the fact that it is possible to produce artificially the special characters of alpine plants grown in the open air, by subjecting lowland species to alternations of temperature comparable to those to which plants are subject at high altitudes. He took a number of familiar lowland plants,—*Trifolium repens, Teucrium scorodonia, Senecio jacobaea, Vicia sativa, Avena sativa, Hordeum vulgare*,—and, choosing in all cases specimens springing from the same stock, grew them in three sets: the first set was kept continually at a low temperature—4°-9° C.; the second was grown under the normal variations of temperature in Central France; while the third set was subjected to very low night temperatures, and to strong insolation during the daytime. As a rule he found that in the third set the subterranean parts of the plant became more developed relatively to the aerial stems; the latter became shorter from an abbreviation of the internodes, more procumbent, and either more woody or more hairy; the leaves were smaller, more fleshy or more hairy; the flowers were produced at an earlier period, and were relatively or even actually larger, and were more brightly coloured. The internal structure of the leaf showed corresponding changes:—the epiderm was less strongly cuticularised; the palisade-tissue became relatively more important; and, in the same leaf-area, the function of chlorophyllous assimilation became more intense. If, as would appear from these experiments, the anatomical and morphological characters of alpine plants are the direct outcome of a response to external conditions, and if these characters are perpetuated from generation to generation, this would seem to afford strong evidence of the non-universality of Weismann's law, that acquired characters cannot be transmitted by heredity.

The number of species of which the flora of the Alps is composed varies, of course, with the view entertained by the botanist of specific limits. The late Mr. John Ball, president of the Alpine Club, the

[1] *Ann. Sci. Nat.* (*Botanique*), vol. xx. 1895, p. 217; *Comptes Rendus Acad. Sci. Paris*, vol. cxxvii. 1898, p. 307.

highest authority, gives the number as 2010, divided into 523 genera, included in 96 natural orders. This is considerably richer than the flora of our islands, notwithstanding our extensive sea-board and great variety of soil and climate. A very few usually maritime plants are, however, found in Switzerland, as the thrift (*Armeria vulgaris* var. *alpina*) on lofty mountains, and the yellow horned poppy (*Glaucium luteum*) on the shores of Lake Neuchatel. Of these species 1117, arranged in 279 genera and 60 natural orders, belong to the upper zone of the Alps. The largest number of species occur in the orders Compositae, Leguminosae, and Gramineae, followed by the Cruciferae, Cyperaceae, and Caryophylleae, each numbering over 100 species. Both in the alpine flora in general and in that of the higher zone, the number of Compositae is nearly double that of any other order, numbering about one-eighth of the whole. Of the Saxifragaceae there are 42 species, of the Primulaceae 36, of the Gentianaceae 26.

The origin of the flora of the Alps is an interesting and somewhat complicated problem. I have already pointed out the great difference between the climatic conditions of Switzerland and those of the Arctic zone. In accordance with what might be expected from this fact, a close examination of the Swiss flora led the two highest authorities on the subject, the late M. Alphonse de Candolle and the late Mr. John Ball, to the conclusion that its nearest connection is not with the arctic flora, but with that of the mountains of Central Asia, especially with the Altai range. The arguments in favour of this view are very clearly brought out by Sir W. T. Thiselton Dyer, in his introductory note to a posthumous paper by Mr. Ball on the distribution of plants on the south side of the Alps, read before the Linnean Society on the 2nd of May 1895, and published in its *Transactions* (2nd ser. vol. v.). According to Mr. Ball, while only 17 per cent of the species found in the Alps are common to the arctic flora, 25 per cent are found also on the Altai range. Still more convincing is the interesting fact that some of the most remarkable and peculiarly alpine members of the Swiss flora (genera or species) are found only on the south side of the Alps, and are distributed at wide intervals throughout a discontinuous mountain chain extending from the Pyrenees to Central Asia; while they are entirely absent from Central and Northern Switzerland, and from the North of Europe. This is the case with species of *Oxytropis*, *Primula*, and *Pedicularis*, and especially with *Campanula cenisia* and its allies, and with the genus *Wulfenia*.

I have touched on only the more conspicuous features of the flora of the Alps. Those who have not yet turned their attention in this direction will find how much is added to their enjoyment of an alpine tour by even a slight acquaintance with its salient features.

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The Scope of Natural Selection.

By J. Lionel Tayler.

A reconsideration of a few of the chief objections which have from time to time been urged against the theory of natural selection may, in view of the more recent development of its principles, be not without some value at a time when test cases to decide the question of use-inheritance and the power of natural selection are being continually brought forward.

In this paper I shall throughout follow Lloyd Morgan, Mark Baldwin, and others in the precise usage of the terms, variation, modification, adaptation, and accommodation.

Variation will apply to changes which are of germinal origin.

Modification will apply to changes which are impressed on the "body" or soma in the course of individual life.

Adaptation will apply to those changes which have been produced by the selection of favourable variations.

Accommodation will apply to those alterations which have been produced by the reaction of the soma to environmental conditions.

We may seek to interpret the facts of organic evolution by resting wholly or in part upon one, or a combination of more than one, of the following assumptions :—

1. That organisms have evolved along definite lines, wholly or chiefly dependent upon the nature of each organism, developing either completely or partially irrespective of the peculiarities of the environment. On this view the more or less unsuitable organisms are simply eliminated, but this elimination is of little or no importance in development, the assumption being that every organism that is not exterminated evolves at its own rate, and that its development is neither retarded nor accelerated by the presence or absence of other organisms.

2. That organisms are modifiable by environment and that modifications so produced are inherited, the hereditary relation being subservient to the action of the environment. This assumption has to be considered under two heads.

(*a*) Accommodations which are the direct result of environmental influence.

(*b*) Accommodations which result from the activity of the organism itself in response to its environment.

It is obvious that these two classes, though not usually so considered, are in reality fundamentally distinct. Class (*a*) includes the only kind of inherited characters that can be truly called acquired. Class (*b*) includes what are in reality merely developments of already existing somatic tendencies, which some biologists believe may, and others that they may not, become germinal. In any case there must be an elementary something which can be developed by use or there would obviously be no development, but rather the formation of a *new* character, and the accommodation would then have to be classed under (*a*). In class (*a*) the influence of the environment in producing a modification is one of primary cause and effect; in class (*b*), on the other hand, the influence of environment is secondary, it is the indirect cause of the degree of the response, but not of the capacity of responding which exists in the particular form of protoplasm itself. Class (*a*) is incompatible with selection, for in proportion as direct modification is able to occur, the less is the necessity of selection, and this direct climatic influence must obviously be also inversely proportional to the power of heredity. Class (*b*), on the other hand, is not necessarily in opposition to the selection theory because within certain limits the more responsive the organism the greater the rapidity of development, selection would become simply more rigorous, the selection value would be raised, the less responsive organisms being weeded out.

There are thus two separate questions in this division to be answered:—

1. Does a direct somatic alteration of structure ever occur as the result of climatic or other physical influence, and if so, how frequently and under what conditions? Do these alterations become germinal? or
2. Do all, or any, somatic modifications to environment arise as developments of a pre-existing element in protoplasmic structure? If so, do somatic responses ever become germinal? For a clear statement of the Lamarckian position it is necessary to determine the relation, if any exists, that class (*a*) has to class (*b*).

3. By the selection of organisms which possess favourable variations, and by rejection of those which have unfavourable, the offspring resulting will tend to reproduce the favourable variations of their parents, and the selection being continued in every subsequent generation, so long as conditions remain fairly constant, there must

inevitably result an organism which tends to vary more and more definitely.

To determine how far evolution has been dependent on one or more of these three factors, it is necessary to estimate—

I. The direct accommodative power of environment over protoplasm, if it exists.

II. The power existing in protoplasm of responding to conditions which favour its activity, and the relation, if any, that somatic response bears to germinal in multicellular organism.

III. Whether the responsive power (II.) or the direct influence of environment (I.) are altered in relation to present by past accommodations, or variations, or both, and if so, the relative importance of the character, intensity, and persistency of these past conditions in producing more or less permanent or transitory modifications or variations in organisms.

It follows from the preceding argument that it is necessary to understand the theoretical capability of each of these three sets of factors to account for the process of evolution, and to endeavour to form some estimate of the probable primitive material from which the present forms of life have proceeded.

In this article I propose to examine this question from three aspects, first, the theoretical capability of natural selection, secondly, some of the chief difficulties advanced against this principle, and lastly, a few of the more general properties of protoplasm and the inferences which these main characteristics appear to justify.

The Limitations of the Principle of Natural Selection.

Ever since the publication of the "Origin of the Species" in 1859, there have been steadily rising into greater prominence, two lines of thought which seem to lead to fundamentally opposite conceptions of the principles which underlie the process of organic evolution. One tendency manifests itself in an increasingly marked disposition to minimise the claims of—use and climatic—inheritance, and to explain the course of evolution by the single principle of selection and certain fundamental properties of protoplasm. The other school of thought tends as emphatically to disregard this selection principle, and to rely on the responsive power of protoplasm and the influence of environment as the main causes of evolutionary development. Some of the members of this school also add to these assumed properties of protoplasm, other innate tendencies by which protoplasm is supposed to be capable of developing along definite lines which are independent of environment. In the one case, the supporters of selection maintained that, as no case of supposed use-inheritance had ever been brought forward which could not be as easily, or even more easily, accounted

for by the single principle of survival of the fittest and elimination of the less fit, they were justified in considering natural selection to be the main or sole principle in species formation. In the other Neo-Lamarckians based their objections to natural selection on the assumption that modifications in nature were always or nearly always definite, that definite modifications were admittedly unexplainable on the selectionist theory, it therefore followed, as nature could produce definite modifiability, without the aid of natural selection, that, unless some special and additional reason could be found for its existence, the selectionist principle must be regarded as wholly subsidiary in nature, and that it could only be regarded as a species-former in the limited field of the domesticated organisms which were under the direct influence of man. Neither position could be regarded as satisfactory, since each school of thought was apparently supported by some facts, while negatived by others. Professor Lloyd Morgan, in an article contributed to *Natural Science* in 1892, altered the whole force of the arguments advanced on both sides by demonstrating the fact that if natural selection acts at all, it must tend, under moderately constant conditions, to produce definite variability through survival of the favourable line of inheritance, and extermination of the unfavourable. This corollary to the principle of selection he has further expounded in his work on "Habit and Instinct" in a chapter entitled "Modification and Variation."

In an article published in this journal for April 1898 I contended that natural selection was capable of producing in the whole organism a general definite variability under relatively constant conditions. I was at that time unaware that Professors Lloyd Morgan and Weismann [1] had both in large part anticipated me.

The former writer's views may be summarised briefly as follows:—

The theory of natural selection, involving as its fundamental principle the assumption that an organism survives solely because it has certain favourable elements in its nature which give it certain advantages in the competition for existence, the less favoured organisms being eliminated, it follows, in so far as parental characteristics are able to influence those of their offspring, that the progeny of successful parents will be likely to inherit a higher average of adaptability to their environment, and as this average adaptability will keep rising so long as selection lasts, it will tend, under more or less constant conditions, to produce more or less definite variability. Definite variability is not therefore necessarily inconsistent with the principle of selection. If it exists only where the conditions are such that the principles of the theory would lead any impartial biologist to expect such definite variability it will be strong confirmation of the truth of the theory in question.

Every living organism may be considered from two aspects—(1) it

[1] In his theory of "Germinal Selection" put forward in September 1895 at Leyden.

tends to develop and maintain its own structure, (2) it tends to reproduce, under suitable conditions, other organisms more or less similar to itself. We have therefore to consider every living form from a somatic and a germinal side. Both somatic and germinal aspects exhibit two tendencies which are differently proportioned in different organisms, (1) to remain constant in spite of variable external conditions, (2) to manifest certain changes of structure. According as one or other of these tendencies predominate the organism will develop and reproduce definitely or indefinitely. In both somatic and germinal development natural selection will tend to favour the requisite definiteness or indefiniteness of structure. The inheritance of somatic characters does not appear to have been established in any one of the many alleged examples; the evidence, therefore, that up to the present time has been collected, would seem to favour the conclusion that if accommodations are ever inherited it is an event of extreme rarity.

Yet in spite of the lack of evidence in support of the inheritance of acquired characters, there seems to be a considerable mass of evidence in favour of the contention that germinal variations often correspond in their tendencies to somatic accommodations.

Definite variability corresponding to environmental accommodation might however be acquired in the following way. It has already been noticed that every organism, both from its somatic and germinal aspects, exhibits two tendencies, one towards definiteness, the other towards indefiniteness; somatic indefiniteness appears to be able to be modified by environmental influences, therefore those organisms whose somatic tendency is predominantly plastic will survive under altered conditions of environment where those organisms of a less easily modifiable tendency will be eliminated. Now if somatic characters rarely or never become germinal, the modifications of the parental organisms cannot be transmitted to their offspring, but those offspring that happened to be endowed with variations in the same direction as the acquired but not transmitted modifications, would start their life with a predisposition favourable to their environment, and therefore favourable to more complete modification of the somatic side of the organism; this tendency being accumulative under constant conditions, coincident variability would arise by the process of selective elimination and preservation, *without* the need for the assumption of use-inheritance, which assumption facts appear to negative.

Coincident variations would thus have a better chance of survival simply because they would be present in the surviving organisms, but the principle of selection would be the same whether the variations were coincident or not.

It follows from the preceding argument that definite variability is a logical necessity, under certain conditions, if the principle of natural selection be allowed to be a factor of considerable importance

in organic evolution. So far all facts point to the conclusion that variations under stable conditions are definite, under unstable conditions indefinite, and this definiteness and indefiniteness occur under precisely those conditions which the theory of natural selection would lead one to expect; hence, unless definite variability can be shown to occur under conditions which selection could not have produced, the facts adduced by the Lamarckian School are favourable rather than otherwise to the Neo-Darwinian position.

To realise how far the theory of selection is capable of explaining the facts of organic evolution, it is necessary to bear in mind the postulates on which the theory is founded.

1. It is obvious that Natural Selection can only act by preserving or eliminating the complete organism. Selection must therefore be organismal. This Darwin and other selectionists have clearly recognised.

2. As the whole organism must survive, if the favourable variation or variations are to be preserved, it follows that certain minor unfavourable variations may also be preserved if they happen to exist in an individual which survives on account of its major favourable variations. And since no individual is completely adapted to its environment, it follows that there must be always a variable amount of residual unfavourable variability in every organism.

3. This residual unfavourable variability may be of considerable utility under changed conditions.

4. Complementary specialisation of parts, as Spencer has shown, is favourable to successful competition, and as it is the whole organism that is selected or eliminated, it follows that any weakness of one specialised part, since it would disturb the balance of all, would be detrimental. The more complex the organism, the more specialised the structures, the more dependent one part will be on the others for its existence, hence a complementary specialising tendency will be favoured by selection, and therefore all struggles of one part of an organism with another will be reduced to a minimum.

It is clear that there must be some underlying criterion which determines whether any given organism shall be selected or not, and that criterion must be the net result of its adaptability to its environment. One organism may conceivably survive, by its possession of a large number of small favourable variations, while another may survive in virtue of a single valuable one, but in each case it would be the *whole value* of that organism which determined its survival. This fact is continually disregarded by opponents of the Neo-Darwinian position, yet this selection of the organism as a whole is the fundamental postulate from which the theory of selection starts. Thus it is not uncommon to read criticisms bearing on the early development of some organ, in which the inadequacy of selection is supposed to be proved by the writer demonstrating, or believing he has demonstrated,

the fact that the particular variation in question must have been too small to be by itself of selection value. In many cases the particular variation would, no doubt, if taken alone be, as the objector asserts, too unimportant to be selected, but as it is the whole organism that is selected, it is not logical to make an artificial separation and study the development of one organ or structure irrespective of the other organs with which it is in nature associated. *Every organ in its evolution must be considered in relation to the whole of the particular organism in which that particular stage of development of that organ is found.* Starting therefore with this fact that the net value of adaptability of the whole organism to its environment must be the basis which determines selection or elimination, it will follow that certain lines of development will result from the application of this criterion. In a series of organisms placed under new conditions, elimination will proceed along lines essential to bring about a proper adjustment to the new conditions. If the offspring of these adjusted organisms merely repeated in their generation the characters of the exterminated as well as of the surviving organisms, that temporary adjustment would be permanent as long as the conditions were unchanged. But since the offspring are produced only by the surviving organisms, selection is continually raised to higher and higher planes of adaptation, and therefore, as long as conditions remain constant, the tendency of selection must be, as Darwin clearly saw, cumulative. He did not, however, apparently see that from this cumulative tendency definite variability must arise out of indefinite.

Selection in direct relation to climatic conditions is therefore of very minor importance, while selection among the members of a species and all forms of inter-organismal selection is of infinitely more importance, since it is this interaction, produced by the offspring in different degrees inheriting the advantages of both parents (both of whom have survived on account of certain advantages), that leads to the cumulative development and never-ending struggle for survival. Darwin came very near to this conception of definite variability when he pointed out that "if a country were changing the altered conditions would tend to cause variation, not but what I believe most beings vary at all times enough for selection to act on." Extermination would expose the remainder to "the mutual action of a different set of inhabitants, which I believe to be more important to the life of each being than mere climate,"[1] and as "the same spot will support more life if occupied by very diverse forms,"[1] it is evident that selection will favour very great diversity of structure.

Bearing in mind this cumulative action of selection it will follow that under constant or relatively constant conditions the struggle for successful living will become more and more selective in character,

[1] From Poulton's "Charles Darwin and the Theory of Natural Selection" (Abstract of Darwin's letter to Professor Asa Gray).

even if the actual number of inhabitants remain more or less the same as when the struggle first commenced. The selection of variations will thus tend to pass through certain more or less ill-defined but nevertheless real stages. In proportion as the struggle becomes intense, either from the number or from the increasing adaptability of the organisms, or both, certain major essential adaptations, which were necessary for the climatic and other more or less comparatively simple conditions, will be supplemented by minor auxiliary variations which in the earlier stages would not have appeared. And still later as more and more rigorous conditions of life were imposed the advantage would tend to rest with those organisms which possessed highly co-ordinated adaptations, since this would entail more rapid responsiveness to environment.

As evolution advances from the unspecialised to the specialised, and higher and higher forms of life come into being, with increasing complexity and specialisation of parts entailing an increasingly delicate adjustment of those parts to each other's needs, the relation of each part to the whole organism becomes of more and more importance, and it follows that selection must become more and more generalised in its action. No single variation could be of service to any of the higher forms of life unless it was in more or less complete harmony with the whole tendency of the individual. The adjustment of parts and their mutual interdependence make it essential for adaptation that the relation of parts be preserved; consequently, correlated minute favourable variations will tend to be more and more selected as evolution passes from the unspecialised to the specialised forms of life. This response of the whole organism should be still more delicate in those forms of life that are continually subjecting themselves to changed conditions; hence this delicacy of adjustment is far more necessary in the higher forms of animal life than in the more stationary plant organisms, and in the developing nervous system of animals we have just the central adjusting system that is required for these conditions. *With evolution of type there will thus be an increasingly definite tendency given to organic, especially the animal, forms of life, if the acting principle of evolution has been selectional.* Selection is therefore able to account for the steadily progressive tendency of life as a whole without calling to its aid any unknown and doubtful perfecting principle.

To summarise:—Natural selection, acting on the whole organism, tends to produce more and more definite tendencies in all surviving forms of life, which tendencies are progressive and continuous in character. Variable conditions, by partially altering the line of selection, induce a temporary indefiniteness. And lastly, the process of selection being itself able to be the indirect, though not the direct, cause of those favourable variations, which it subsequently selects from, is able to dispense with any subsidiary factors, provided it has a

certain number of elementary properties of life which afford sufficient material to work with.

Objections to the Theory of Natural Selection.

Keeping constantly in view the leading principles of the selection theory I believe it will be found that the facts adduced by the more scientific opponents of this theory can, when the importance of the corollary put forward by Lloyd Morgan, and after him by Weismann, is considered, be easily accounted for, and that as they then fall into line with its legitimate deductions increase the strength of the theory by showing it to be a more and not less important principle than Darwin and even Wallace were led to believe.

1. *Variations are definite and not indefinite in nature.*—This objection has already been met in the preceding part of this article, and as selection is able to explain the indefinite variability which arises from variable conditions, crossing, etc., and the constancy of type from rational inbreeding, it is in more complete accord with facts than any mainly Lamarckian or Orthogenetic theory.

2. *That Natural Selection cannot be the cause of New Characters —The alternative must be present before the selection can commence.* If any character or variation can be shown to have been produced which differs qualitatively, not merely quantitatively, from its parental forms, which is not to be explained by incomplete development, atavism, or degeneration; if any variation can be shown to arise, which has not some pre-existing though less or more differentiated counterpart, it would form an objection of considerable magnitude. But as no case of the kind has been put forward which Neo-Darwinians have felt bound from the strength of the case to accept, this objection may be disregarded until such case arises.

3. *The difficulty of the chance variation appearing at the right moment* is largely met by the fact that selection tends to induce determinate variability; this objection is still further weakened by the fact that even relatively rapid changes in nature are, as a rule, long in proportion to the life of the individual, and afford considerable opportunities for selection working through somatic accommodations and later coincident germinal variability to produce the required change.

4. *That the earliest forms of variations must have been too small and insignificant in character to be of selectional value.*—This objection appears to me to be one of the most weighty of all the objections which have been raised to the selectional hypothesis, and it is further an extremely difficult objection to satisfactorily reply to; first, because it is almost impossible to say in what form of organism the earliest variations appeared, and without this no judgment on the value of any small variation can be of use; secondly, it is equally essential to know the kind of environment which such an organism

was living in; and lastly, if we were fully acquainted with the character of the organism and its environment it would still be difficult to form any adequate opinion on the value of such a variation, owing to the fact that this apparently simple organism would differ so widely from our own functional activity and life that any conclusions formed on comparative methods of testing its powers, etc., would be extremely likely to be fallacious. If, however, we keep in mind the facts that (1) the whole and not merely a part of the organism is selected, and that, therefore, each variation does not require to be of the same value as if selection depended on it alone; (2) specialisations are largely quantitative, between man at one extreme of development and a simple unicellular organism at the other, the difference though very great, is mainly due to the fact that man is a huge multicellular colony; this difficulty will be much simplified. To estimate the qualitative difference it is necessary to endeavour to determine the specialisation of an individual cell in one of those collective specialisations or organs: the difference between a cell in, for instance, the cerebral cortex of man and the character of an amoeba is no doubt great, but the amoeba reacts to stimuli, though in a less specialised form just as the cortex cell does; in the same way the reaction to light in the mammalian eye is not a new development—it has its beginnings in the preference for light or darkness shown by many unicellular organisms. These two points that selection is organismal and that specialisations are as, or more, largely quantitative than qualitative, weaken if they do not abolish all those difficulties to natural selection that are founded on this objection, and it is further necessary to recollect that no specialisation has yet been found which has not a primitive counterpart in the earliest known forms of life.

5. *The Imperfections of the Geological Record.*—This is obviously a much less important objection than the preceding one. The very large areas of the world that have yet to be examined tend very much to weaken any objection founded on imperfections and absence of links. And as with increasing research these missing links are being steadily filled in, it follows that this objection has become weaker and not stronger with advancing knowledge.

There are, however, certain points which it is essential to recollect in any consideration of the imperfections arising from this cause. Lloyd Morgan has pointed out that, as the tendency of natural selection is to favour, under appropriate conditions, definiteness both in the soma and in the germinal structures, the geological record should not be expected to provide evidence that does not correspond to this definite line of development.

There is also another point which does not appear to me to have been sufficiently emphasised. In the earlier part of this paper I drew attention to the fact that Darwin considered the mutual action of a different set of inhabitants arising from the birth of a new generation

to be of more importance than the mere conditions of climate, etc., and inasmuch as climatic selection will largely cease acting as soon as organisms, capable of surviving at all under these altered conditions, are produced, it follows that inter-organismal action, which is continuous, must be of more importance in species formation and differentiation of structure. But as organisms which cannot survive under these altered conditions will be eliminated, it follows that the more obvious structural changes will be largely produced by this temporary climatic selection, and this form of selection will be remarkably rapid in its action relatively to the inter-organismal selection. Hence the obvious structural changes induced by climatic selection will have less chance of leaving a geological record behind them than the less obvious variations induced by inter-organismal selection. For this reason certain imperfections in the record are likely, and should be expected, to arise.

6. *That the period of time is too short for such great alterations of structure to have taken place.*—As the rapidity or slowness of structural alterations will depend on the local surrounding conditions, it follows that, until some fairly complete record of these local conditions is obtainable, no objection as to time limit can be logically raised.

7. *The co-ordination of parts necessary for the development of favourable adaptations.*—Spencer has pointed out that co-ordination of many parts to form one adaptation is based on a different principle to the cumulative results of many different variations each of which is of selective value, and urged that natural selection is powerless to explain this co-existent adaptation.

Wallace, in referring to this subject, says:—"The fact, that in all domestic animals, variations do occur, rendering them swifter or stronger, larger or smaller, stouter or slenderer, and that such variations can be selected and accumulated for man's purpose, is sufficient to render it certain that similar or even greater changes may be effected by natural selection, which as Darwin well remarks 'acts on every internal organ, on every shade of constitutional difference, on the whole machinery of life.' The difficulty as to co-adaptation of parts by variation and natural selection appears to me, therefore, to be a wholly imaginary difficulty which has no place whatever in the operations of nature."[1] This criticism does not appear to me to do justice to Spencer's objection; he would no doubt agree with Wallace that these accessory variations can be developed by selection, but he would go one step farther back and ask why it is that the accessory variations happen to be there to be selected from at all. He would agree to the fact that selection must act on the whole machinery of life, but he would still urge that he is unable to see how it is that all these numerous accessory variations which are necessary to the working

[1] "Darwinism," p. 418.

of one variation happen to be present at one and the same time. His difficulty therefore does not appear to me to be answered by Wallace.

Weismann,[1] admitting the objection of Spencer's as having a real existence, attempts to answer it by the tendency of natural selection itself to induce definite variability. This answer does not seem to me to be much more satisfactory than Wallace's, for the point of the argument is, that as the accessory variations are necessary to the proper working of the primary they must be present from the first selection, and as determinate selection can only appear after selection has been continued for some generations it must be unable to explain this occurrence of co-ordinated parts which occurs prior to the action of selection.

Mr. Lloyd Morgan in the December number of *Natural Science* deals with this difficulty in a manner which appears to me to be much more satisfactory. We have seen in the brief summary of his views that he draws an important distinction between somatic response to environment and the selection of germinal variations, that under altered conditions of environment he considers somatic plasticity to be one of the principal determining causes of selective preservation, and as he admits the action of use-modification on the somatic structures, those organisms whose somatic structures are sufficiently plastic to allow of this newer co-adjustment to the newer conditions will survive on account of their plasticity, and this will continue to happen over one or more generations until chance variations happen to make their appearance in the same direction as the environment, then the offspring of this organism or these organisms will start life with a slight favourable predisposition to their environment, which in addition to somatic plasticity will give them a slightly better chance than those without this predisposition, hence by the fostering power of body response a co-ordinate structure might be formed through cumulative co-incident variability. This objection therefore does not apply to the theory of Natural Selection modified as above.

Keeping in view this theory of co-incident variability, there is another consideration which will also tend to weaken this objection. As selection must be from the first organismal, and as adaptation to climatic conditions must be absolute, as far as it is capable of exercising a selective action, a certain common tendency will be present in all more or less similar organisms living under these more or less similar physical conditions. This primitive climatic basis will give a certain direction to the subsequent inter-organismal selection, and we have seen that with progressive evolution the necessary specialisation entails an increasingly definite tendency in the organism as a whole, owing to the increasing dependence of one part on another: hence it will follow that all variations will tend to become increasingly co-ordinated as they become increasingly specialised, and they will also become increasingly so as we pass from the lower to the higher forms.

[1] "Germinal Selection."

There will thus be very little tendency for incoördinated variations to appear, and this tendency will diminish with evolution of type.

8. *That organisms not uncommonly exhibit a more perfect organisation than their environment demands.*—This statement is frequently associated with other similar objections, some of which, such as definite variability, and varying degrees of capacity to vary in different animals, have already been met; it is also asserted that animals sometimes manifest at the earlier periods of their lives a higher condition than at a later period, and that this higher earlier condition cannot be explained by any assumption of reversion in the later stages of growth, thus it is asserted that the infant ape is much nearer to man than the adult ape, etc.

All these assumptions have as a basis the conscious or half conscious belief in some unknown internal force which is capable of producing evolution of type independently of environment. To Lamarckian and selectionist theories alike any such force, were it proved to exist, would be largely fatal.

It has been shown that an increasingly definite tendency in organisms evolved through the principle of natural selection is what on theoretical grounds one would be led to expect—that the preservation of a definite relation of one part to another becomes of increasing importance with increasing specialisation. That this is actually the case, the facts associated with "internal secretion" in man and the higher mammals clearly prove. The thyroid, kidney, liver, pancreas, testes, and ovaries, etc., have been shown to exert some remarkably important influence on the nutrition of the whole body, and this influence in the case of the thyroid, and less certainly in other organs, has been found to be produced through the throwing off of certain products into the circulation which are necessary to the metabolism of the whole body.

On any theory of complementary specialisation of parts such facts are easily understandable. A chemical circle of nutrition would be the most economical way of maintaining tissue activity; if each organism can act chiefly on some particular substance, one organ or tissue requiring a more complex food material than another to carry on its metabolism, then the waste product of one organ might be used as a food product by the next in this food series, until the last organ of this series, having obtained all the energy from this material, excretes this simpler substance, which cannot be further utilised by the body, into some channel where it is got rid of. Some such hypothesis is necessary to explain the facts, and the increasing series of progressively simpler products, although still incomplete, that have been obtained, which are allied to uric acid and other substances, lends considerable support to this theory. There would be thus a serial specialisation of food supply among the tissues of each organism which would be as economical as the specialisation of food supply among

individual organisms competing in nature. Now this close relation of one part to another which is characteristic of the adult organism is also equally characteristic of the developing one, and, keeping this sequence of nutrition in view, each organism starting from a more or less quantitatively generalised substance, evolves to quantitatively specialised structure, in the building up of which every antecedent stage of development is necessary, and forms a basis for the later stages, it will follow that a definite, regular order will be developed; *and hence definiteness in growth and development is as essential as definiteness in the relation of one part of a specialised organism to another.* That this necessary sequence in development is no mere unsupported conjecture is shown by the fact that the relation of parts alters with growth, an organ occupying a first place in activity at one period may become second or third at another, this alteration of the relative size of different organs to the whole body at different ages must be of some value to the whole organism or it is unlikely that it would be perpetuated; the thymus gland affords a typical example of this—it appears in some way to be associated with development, it reaches its maximum size in man about two years after birth, and then slowly shrivels up; the presumption is that at that period it had some function to perform which ceases to be required. If we assume a metabolic sequence in structure we explain this varying relation of parts, and we explain its definite character, and this sequence, as in other specialisations, would be subject to the influence of natural selection; so far preservation of different stages of growth can be easily accounted for on a selection hypothesis if this necessary chemical sequence is assumed, and without it no theory has as yet explained the facts.

There thus remains from this objection only those cases where there is an apparent or real foreshadowing of a higher evolutionary type. Now before this foreshadowing can be used as an objection, it has first to be determined how far it is real or not. It is well known that the ovum of one animal resembles another considerably, and that the higher animals, as they pass through successive stages of their development, resemble more or less incompletely certain lower forms of adult organisms, and this has led to the assumption of the recapitulation theory. Were it possible to reverse the order of evolution and proceed backward, we should find all types converging towards unity, and while this applies to the whole line of development, it equally applies to lesser portions of it. As the infant ape is less specialised than the adult ape it is more likely to present similarities to man, not on account of an actual foreshadowing, but simply because, being more generalised in structure, it is less easy to mark off differences; for precisely the same reason a human child might appear nearer to some ideal and higher type of man.

Until this fictitious resemblance is dealt with this objection can be disregarded. Further, as many biologists have already pointed out,

there is always a certain excess force, which would be fostered by selection, sufficient to provide for emergencies.

9. *Rudiments and their disappearance.—It is assumed that there will come a point where the rudiment will be of such slight significance that it will no longer be of selection value, hence it is urged that the fact that rudiments do tend to completely disappear, is against any purely selectionist principle.* Leaving out of consideration the possibilities of reversal of selection, panmixia, etc., it appears to me that there is a comparatively simple cause for this disappearance. George Henry Lewes, Wilhelm Roux, and more recently Weismann, have all fallen back on the assumed necessity of applying the principle of selection to the several parts and specialisations of the individual organism, in addition to the action of selection on the whole organism. The last writer in particular, in his "Germinal Selection," suggests that a struggle among the different parts of the germ-plasm may account for the complete disappearance of rudiments, this germinal selection thus supplementing the action of panmixia, personal or organismal selection, etc. Now the necessity for increased co-ordination of parts with increasing specialisation, entailing, as it necessarily must, an increasing mutual dependence of each part on the others, must lead as the type advances to diminished opportunity for any struggle of parts in the organism, consequently if such a struggle exist at all it must be limited to the most undifferentiated organisms. I do not therefore see how this principle can explain the disappearance of rudiments in any of the more specialised organisms, hence it does not seem to be sufficient answer to the above-mentioned difficulty. In the development of the individual we see a disappearance of structures, which appear to become with advancing development useless, almost parallel to the gradual disappearance of rudiments, etc., in the history of the species evolution. And a common explanation for both of these series of phenomena can, I believe, be satisfactorily found in the known facts of nutrition. Growth of any tissue would seem to depend on three conditions, a stimulus of the part adequate to promote functional activity, a proper food supply, and efficient removal of products produced by that particular tissue's activity. There is abundant evidence to prove that a tissue tends to degenerate if its own excretory products are not removed; the evil effects produced by fatigue products in muscle and other tissues on the activity of the tissue itself prove that this factor must be of great importance wherever it is found to occur. Just as the growth and development of bacteria is interfered with, and finally altogether checked by the accumulation of products of their own activity, so a tissue in the higher organisms has its activity impaired and its power lessened when for some reason diminished elimination of its own metabolic products occurs. Now both in the development of the individual and the race we see an alteration of structure, a gradual transition from the less to the more specialised,

and in this gradual transition there must be, as I endeavoured to prove in my answer to the last objection, an alteration in the line of functional activity of the parts, and that, owing to this fact, a tissue that was necessary in the earlier stages, became less and less so as specialisation advanced, the whole tendency of the specialising organism being continually and increasingly against the earlier, less specialised, stages. It will thus happen that every structure which is becoming useless owing to its deficient specialisation, whether in the history of the race or the individual, will have two adverse sets of conditions to contend with—one defective elimination of its own tissue products, owing to its becoming increasingly removed from the growing organismal specialisation of food products, while secondly, for this same reason, its own food supply will become less and less suitable. This theory would apply equally to germinal and somatic development and atrophy of structure; there would thus, through the alteration of functional activity of the whole organism, be brought about elimination of all structures not in the line of evolution, and therefore organismal selection alone, if this theory is sound, would be able to explain the complete disappearance of rudiments, the various forms of development and atrophy, without calling to its aid climatic inheritance, panmixia, and germinal or any other form of particular selection.

The only two other important objections against the principle of selection are (1) those cases where it is assumed that automatism produced by habit has become hereditary (instinctive),[1] an assumption which an examination of the facts does not appear to warrant, and (2), those cases which are supposed to be examples of experimental demonstration of acquired inheritance.

In the best known of these experiments, particularly those performed by Brown-Séquard, we have certain facts which appear to show that under very exceptional conditions somatic injuries may affect germinal structures. Assuming that reliance may be placed on this interpretation of these experiments, an interpretation which future facts might conceivably negative, there are other facts associated with the relation of environment, alcohol, etc., to crime and insanity which would seem to offer some slight confirmation of this view. If further investigation proved the possibility of somatic responses affecting occasionally the germinal structures, it would only affect any theory of heredity which was based on the assumption that somatic and germinal elements were completely isolated. The purely selectionist position would remain intact unless direct climatic accommodation could be also proved to be a factor of importance. The objections to the selectionist theory do not appear, therefore, when examined, to be valid.

[1] See Lloyd Morgan's "Comparative Psychology" and "Habit and Instinct," and Mr. E. L. Thorndike's experiments.

(To be continued.)

Stray Impressions of the Marine Invertebrates of Singapore and Neighbouring Islets.

By F. P. Bedford, M.A.

Nearly all the facts mentioned in the following account are probably well known, but so few English naturalists seem to have visited the Malay Peninsula with the object of studying its marine invertebrate fauna, and my own preconceptions of marine tropical life derived from lectures, books, and specimens, which more or less faintly recalled their original form and colour, were so vague and so often erroneous, that I cannot help thinking that there may be many who, from lack of the opportunity or possibly the desire to travel in the tropics, may be in a similar predicament. If this is so, a few of the impressions produced on one's mind may not be entirely devoid of interest.

No doubt all who are interested in the subject will have read such books as Professor Hickson's "Naturalist in North Celebes" and Professor Semper's "Animal Life," books written in a most suggestive and lucid style, made the more convincing by the intimate practical knowledge which the authors possessed of the animals they describe. I cannot of course pretend to any such knowledge on my own part, and I would not venture to traverse ground which has already been so admirably reconnoitred, but there is a purely superficial aspect of the subject which some months' collecting in the neighbourhood of Singapore has impressed on my mind, and which may be worth attempting to describe before it has become obscured by the details which assume an increasingly prominent position in one's thoughts the longer one collects.

One of the first impressions produced when one either turns over stones or digs at low-tide, or dredges or trawls in the sea beyond, or examines the results of surface tow-nettings after dark, is the marked similarity of the fauna to that of our English coasts. At or near the surface at night are Appendiculariae, Copepoda, Malacostracan larvae, Chaetognatha, Medusae, Siphonophora, and Ctenophora, many of which, to the naked eye at least, are quite indistinguishable from those which might be obtained in a similar way at Plymouth or Port Erin, such forms as Heteropoda, Pteropoda, and the larger pelagic Tunicates being by no

means common as a rule. In the dredge are obtained, according to the depth of water, nature of the substratum, strength of currents, etc., different forms of invertebrates which, as a rule, recall at once some English genus, the most noticeable being perhaps the Sponges, Hydroids, Gorgonians, Polyzoa, Ascidians, and the five groups of Echinoderms. The littoral fauna is not at first sight strikingly unusual except in those places where the reef-building corals flourish; here undoubtedly a surprise awaits any zoologist who sees them for the first time. Often as he may have seen the beautiful photographs in Saville-Kent's well-known work on the Australian Barrier-reef or collections of coral such as are exhibited at the Natural History Museum at South Kensington, or often as he may have read the accounts of Darwin, Dana, Murray, Semper, and others on the formation of coral reefs, he will hardly, until he is brought face to face with the reality, have been able to form a mental picture which at all adequately represents the actual charm and beauty of the living coral, reposing calmly "like a flower garden" (as I think Moseley described it) beneath the seemingly unnatural transparency of a tropical sea.

In these shallower waters, which rarely exceed a depth of 30 fathoms, the reefs differ considerably from those usually described, and a short account of them may not be out of place.

The reef-building corals form a fringe which is by no means always continuous round the islets or on the margin of the coast; on the latter especially there are extensive tracts covered with sand or mud, and with occasional mangrove swamp, but totally devoid of reefs, coral being represented by small clumps distributed very sparsely at intervals of often several yards. In places where the reef is present, its distance from the shore varies from a few yards to half a mile or more, and in many cases no part of the reef proper is dry at low spring-tides; the actual width of the reef itself is also very variable, but rarely exceeds about ten yards; on its outward edge it slopes somewhat abruptly to about five or six fathoms, and then more gradually seawards. Between the reef and the shore there is nearly always a flat covered with mud, and very often with an abundant growth of brown sea-weed which harbours a large fauna. This mud flat is very nearly level, and at lowest spring-tides there is left about a foot of water in the deepest parts, the highest portions of the "flat" being just dry. The mud sometimes extends nearly up to high-water mark, but as a rule it is separated from the land by a belt of sandy or rocky ground, or occasionally by projecting volcanic rocks excavated by the sea into hollows, which on the retreat of the water form tide-pools, and contain numerous nooks and crannies in which molluscs, crabs, and other animals find a hiding-place. Here at any rate at first sight the naturalist will readily admit that he might be on English ground. As he looked more closely he would probably see large fleshy Alcyonarians abounding on the mud-flat, and to some extent replacing our anemones, the latter being only

locally common; he might also see large Holothurians basking in the sun, either stationary or crawling slowly over the mud, but the commonest groups would be those that he was already accustomed to. Hermit-crabs abound everywhere, and at night the shore will sometimes be almost covered with them; crabs and prawns shelter themselves in crevices or under stones or in the sand, and Spatangids, Chaetopods, and Gephyreans make their burrows in the sand or rocks; limpets, too, of a diminutive size it is true, but still obvious limpets, stick to the rocks with the same tenacious grip as elsewhere, and obviously fill the same place in the economy of nature; our common littoral Gasteropod genera, such as *Nassa*, *Purpura*, *Littorina*, *Trochus*, etc., are represented by forms closely similar both in form and habits, and many of the species seem to have extremely variable coloration as on our own coasts; in fact it would be difficult to name any characteristic difference. Polychaet tabes project from the surface on nearly every sand-flat, Lamellibranchs abound in the mud and bore into rocks and wooden landing-stages, Nudibranchs of brilliant colours, together with Polyclads, creep about on stones and sea-weed, and even the abundant *Periophthalmus* which forms so marked a feature of the littoral fauna as it bounds over the surface of the pools, or rests on some adjacent object just above the water, is after all only a goby, such as every boy-naturalist delights to hunt at home.

The conclusion thus seems forced on our attention that the broad features of marine life, the modes of adaptation of different groups to their inorganic environment, and the modes of life adopted in their mutual rivalries of offence and defence, are to a very considerable extent independent of geographical position or climatic influence, and what is perhaps more surprising, they would seem to be independent of the marked differences which undoubtedly exist among the higher vertebrates. The presence of numerous kinds of tropical sea-birds, of sea-snakes, of crocodiles, and of a host of curious fish seems to have made a scarcely appreciable impression on the habits of the lower forms; and from what we know of fossil fauna, commencing from the *Olenellus* and other faunas of the earliest fossiliferous rocks which have retained the imperfect relics of but a few of their once living inhabitants, it might be surmised that from that time onwards these same broad features have persisted all the world over, altered but slightly from time to time by the subsequent evolution from some of them of the Decapod Crustacea, Vertebrates, and other "higher" forms. No doubt, too, in a similar way the exclusively tropical forms, among which we may perhaps regard the reef-building corals as in this respect the most important, have led to modification of the animals dependent on them, but from a superficial point of view at least, the crabs, prawns, Cirripedes, Lamellibranchs, and Holothurians that live associated with them do not differ very considerably from their allies which are surrounded by other environments.

By most writers on tropical zoology much stress has been laid on these modifications, and we have all repeatedly been told of the brilliant coloration of tropical marine animals, of the way in which hermit-crabs wander far inland, of fresh-water crabs, etc., but to my mind the resemblances are much more striking than the differences, and all that I have attempted in the present article is to give some idea, necessarily very imperfect, of the general impression produced when collecting for the first time in these waters, and if it is thought that more detailed facts are required before any generalisations are possible, I can only hope that at some future time I may be able to contribute my mite to the common store.

Raffles Museum,
Singapore.

A Theory of Sleep.

By Professor A. L. Herrera.

Sleep is not peculiar to man, for it presents itself in every organism. "Protozoa themselves sleep," says Milne Edwards, and sleep must, therefore, have quite a general cause. Some substances (narcotics, anaesthetics) provoke sleep either by dehydration or by producing congestion in the nervous centres, etc. On the other hand, sleep does not invade every organ in the same manner; it presents itself sporadically in such organs as happen to be extremely tired, or in those that are not well fed. It does not, in short, essentially differ from hibernal sleep.

Let us seek then for a philosophical explanation comprising every particular case and requiring no suppositions nor vitalistic theories. I find but one entirely general cause: the delay of the protoplasmic currents in which life consists, as I stated in a special paper on this subject.[1]

The Sleep of Plants.

In animals sleep is characterised by the flaccidity of their locomotor organs, whilst leaves remain in their nocturnal state on account of a very remarkable rigidity that seizes them. Linnaeus once received from Prof. Sauvageau of Montpellier a shoot of *Lotus ornithopodioides* L., which began to flourish in a hot-house at the garden of Upsala. The great botanist examined the flowers directly they opened and observed that they disappeared on the same night. He believed at first that they had been thoughtlessly cut away, but had to acknowledge his mistake next day, as the disappearance of the flowers at night depends completely on the close approach of the adjoining leaves which form a kind of shelter for them. This observation afforded cause for fresh investigations, and it was discovered that every species of plants opens and shuts itself at an appointed hour, etc.

Explanation.—"The motor dilatation occurring in some leaves at the base of the petiole is due to two antagonistic factors, the one tending to raise the leaf, the other trying to bend it, but the former, being by nature the weakest, acquires an additional force whenever

[1] "Protoplasmic Currents and Vital Force," *Natural Science*, April 1899.

light and heat, endowed with a certain degree of intensity, produce an abundance of sap in the cells which increases the turgescence: it can then resist the action of the opposite factor." In short, this is but a mechanical effect of the delay of the nutritive currents coming up the leaves.

Dreams.

These vary both in essence and degree according to the state of the dreamer's circulation. Some hygienic exercise or the repetition of a lesson may probably cause certain neurons to go on moving during sleep. But when they have worked too actively in the course of the day they are liable to be utterly drained and exhausted when night comes, and when such is the case there may be dreaming of the facts that brought their fatigue about. An assiduous exercise of the neurons may facilitate their continuous development and action (*e.g.* in the student dreaming about his examinations again and again). Contrariwise, the absence of new impressions, or a limited exercise during the day, will allow the uniform rest of all the neurons and a thorough absence of nightmare (husbandmen).

Fixed ideas lead to madness, perhaps on account of an atrophy of the inactive parts, some limited congestions, hypertrophies, etc. This is no business of mine, but I must state that the possibility of the functions of some cerebral centres being accomplished independently is made manifest during sleep. This means that certain neurons become associated in an abnormal way, extending themselves too much, and that diseases of mind, disordered neuroplasmic vibrations, are not inhibited by the more powerful vibrations of sound judgment, this being then peacefully slumbering.

Causes of Sleep.

Theories on this subject are by no means wanting, but they concern man only; they are not capable of general application, and leave the innermost mechanism of the phenomena unexplained. I admit, if necessary, the action of poisons and that of the secretions of the organism accumulated during the day, but chloroform and hypnotism work in the same manner. Whether the brain be congested or whether it be anaemic, its functions are deeply modified on account of the delay of the currents. Moreover, the lowest animals (Protozoa) sleep and wake in accordance with the conditions of their activity.

I believe, therefore, that sleep originates, either in man or infusorian, in a delay or slowing of the protoplasmic or neuroplasmic currents, due to refrigeration, lack of nutritive fluids, congestion or anaemia. Everything grows wearied. Everything bores and is bored. Both Bütschli's foam and my protoplasmic mass made by synthesis, cease from visible movement after a certain period of activity. Briefly,

it is a mere question of provisions. When the oxidisable ferment is spent, when zymoses decrease, and almost all the material carried from the external to the internal medium is wasted, it is but natural that movements and currents become slower and slower. The organism is then said to be sleeping. And how many degrees there are from the simple yawn and somnolence to the drowsiness of a worn-out and fatigued traveller! But currents do not cease entirely—death is not the issue. The transport of materials is slowly continued from the digestive apparatus to the recesses of the organism, from the outside to the inside.

In wakening organisms oxidations increase little by little (just as in Bütschli's plasm when heated); the current is augmented (as in Herrera's plasm when it receives a slight addition of peptone); the reagents in the laboratory begin to bustle, the forge's reverberations swell, and the hymn of work grows louder and louder until it finally attains the pitch of thunder. Bear this in mind, that the act of waking is a slow one, having many degrees and shades. At the break of day our sleep is light, and we begin lazily to stir ourselves without even opening our eyes, whilst we remain fluctuating in a pleasant languor.

Keep this rule in mind; whenever there is a cause, be it *y*, *z*, or *n* that modifies nutrition, sleep will increase in the exhausted convalescent, in the newly-delivered mother, in the child endowed with an exceedingly active circulation, in the inhabitant of the tropics whose salts and water are perpetually drained by the everlasting cupping-glass of climate, in the traveller, in the drunkard, in Bütschli's "artificial protoplasm," and in my own when seen under the microscope at their respective periods of activity and asthenia, in the glutton who ingests and absorbs large quantities of nutritive material, and in the youth who has provoked great waves of commotion which propagate themselves through vast nervous territories. On the contrary, old people and sedentary persons sleep both badly and scantily, as they stand in waiting for death.

I do not admit, O metaphysicians! the existence of any hard and fast line between sleep, this anaesthetic of life, and waking. I do not believe, O vitalists! that an organism can ever be either completely awake or completely asleep. There is always something living, one organ sleeping and another palpitating. A goose never happens to shut both its eyes at once. My own heart has at no time slept as my brain does; it hardly ever rests, poor perpetual sentinel! And you, O muscles? We yawn, wake and work too. There are some disinherited, beggared organs sleeping in ascetics. Yet, there is a weak and slow nutritive current even there.

I deny, then, any hard and fast line; there are no barriers between sleeping and waking, just as there are no absolutely separated and divided things in nature, whether stars or organisms.

But the day comes when both the currents and the general

irrigation cease; my Amazon is dry and the pale brain can drink no more from the drained internal stream. True sleep comes then. Cadaverous decomposition is, however, accompanied with some slight currents which are neither protoplasmic nor co-ordinated.

About some Particular Cases.

(*a*) *Trance.*—This consists in the diminution of certain currents, and is a more limited sleep than that effected in normal conditions. Hypnotizers avail themselves of several means of fixing or inhibiting currents (compression of the eyes, staring, gazing at a brilliant object, or suggestion, that is, the inhibiting action of the will on some nervous currents of a particular sort).

(*b*) The sleep of nocturnal animals in the course of day is related to the action of light. In Mexico bats have been observed to issue from their dens during eclipses of the sun; gnats flutter in rooms during day-time as soon as all doors are shut so as to leave the apartment in the dark. Everyone has seen that owls close their eyelids whenever a vivid light strikes them.

(*c*) *Muscular Relaxation during Sleep.*—I believe that muscular contractions are due to certain changes in the volume of the protoplasmic alveoli. Rhumbler has demonstrated that such is the possible cause of mytosis, and that the rows of small alveoli, when these are partly emptied, diminish in volume and exercise a strong tension on the centrosomes. The dynamical influence of those changes being wanting when nerves are sleeping, and there are no waves nor modifications in the intra-alveolar pressure, it is clear that muscles must relax.

The same happens in several pathological cases under the influence of fatigue or of certain depressing emotions, etc. This means that I suppose nervous waves to provoke the passage of the alveolar enchylema into the protoplasm of the muscles either by the mechanical action of the shock or by an increase of hydrostatic pressure. I do not deny that the latter have the structure and elasticity required. It will be remembered that the muscular wave moves along the muscles of ants in such a way that it is observable under the microscope. This could not be the case in a homogeneous liquid.

(*d*) Naturalists faithful to the old school would find a remarkable "harmony" in the following fact:—

According to Van Beneden the intestinal worms of bats enter into a period of hibernal sleep at the same time as their hosts. That is to say that the deep protoplasmic currents are delayed both in the host and its parasite by lack of nourishment.

Summary (concerning every living thing).

Nutritive currents are endowed with a very great velocity in active life.

Nutritive currents (sap, blood, protoplasmic currents) are periodically delayed by the want of the reserves expended during the day, and the result is sleep.

The same currents may be less active during the day on account of inaction or of some other cause, and the result is somnolence. This may also be ascribed to nervous excitation.

Currents delayed by the constant action of cold—Sleep in winter.

Currents delayed by an excess of external heat—Sleep in summer.

Currents delayed or even utterly prevented by lack of moisture—Latent life.

General co-ordinated currents definitely stopped by coagulation, poisoning, hemorrhages, asphyxia, etc.—Death.

An Artificial Schematic Organism.

The principal varieties of sleep, life, and activity may be illustrated by an organism which I have constructed. It can be modified and perfected in a thousand ways, and several may be brought into connection. It consists of a damp chamber bounded by walls of cement and gypsum, or a paste of carbonate of lead and linseed oil (skin) with efferent capillary tubes (excretory apparatus). Between the two glasses and the two partitions there are big drops of Bütschli's cytoplasm or "artificial protoplasm" and water. In the middle stands a digestive apparatus formed of thin caoutchouc or of a snake's lung; two tubes of glass serve to keep it open at the ends, and it is made narrower in the middle; it receives food (peptone, water, and some sugar solutions) through one end and expels it through the other. For this purpose the mouth is covered after filling the cavity. The whole is afterwards heated by means of a small oil-lamp, and then cooled or dried, whilst the currents and the osmotic phenomena, the deposits, concretions, etc., are observed. The internal currents and movements are stimulated or paralysed according to the conditions mimicking those called vital. As respiration cannot be imitated, the heat afforded by oxidations may be replaced by that furnished by the small oil-lamp: after all it is exactly the same thing. The two glasses being difficult to unite they may be replaced by Vierordt's glass-box or haematochrometer.

Note.

In a relatively young country, such as Mexico, investigations concerning General Biology are very difficult. Science has fructified here

only for the last twenty or thirty years, and that beneath the shade of a most complete and dispiriting peace. There is a lack of teachers, books, laboratories, and intellectual vigour—the latter chiefly. Consequently, although it would make me happy, I dare not beg for the protection of the learned foreign corporations, considering myself unqualified for it, but I will at least beg that some indulgence be shown regarding the imperfections with which all my works do surely abound.

Mexico, *April* 30, 1899.

FRESH FACTS.

PUMP BENTHOS. W. P. HAY. "Description of a new species of subterranean Isopod," *Proc. U.S. Nat. Mus.* xxi. 1899, pp. 871-872, pl. lxxxvi. Forty or fifty specimens were obtained from an old well in Irvington, Marion County, Indiana. They were evidently strictly aquatic. The pump in the well drew water from the bottom, and the animals could be obtained only by vigorous work. After capture they lived for some hours in a jar of water, crawling about on the bottom, very much after the manner of *Asellus*. While in the water the swimming feet gently moved up and down with a fanlike motion. Several of the females carried eggs, six or eight of which were sufficient to fill the brood pouch. The species is named *Haplophthalmus puteus*. Other species of the genus are inhabitants of moist situations, such as decaying leaves and wood, in various localities in Europe. It is also closely related to *Scyphacella* (*Haplophthalmus?*) *arenicola*, which has been found burrowing in the sand in a number of localities along the Atlantic coast of North America.

A ZOOLOGICAL PUZZLE. WILLIAM MORTON WHEELER. "The Life-history of *Dicyema*," *Zool. Anzeig.* xxii. 1899, pp. 169-176. The author's observations suggest a new conception of the life-history of *Dicyema*, which has been for a long time a zoological puzzle. He believes that the same *Dicyema* is at first a "nematogen" (or female produced from parthenogenetic ova and producing other females parthenogenetically), and then a "rhombogen" (producing what are called infusiform embryos which arise from fertilised ova and are really males). "As in so many other cases in the animal and vegetable kingdoms the males make their appearance when the conditions of life become unfavourable, viz. after the kidney (of *Octopus*) is well-peopled with Dicyemids and food is less abundant." Mr. Wheeler believes that the structural and developmental peculiarities of the Dicyemids entitle them to a more independent rank than that of an appendix to the flat-worms.

HOW YOUNG DUCKMOLES GET MILK. V. SIXTA. "Wie junge Ornithorhynchi die Milch ihrer Mutter saugen," *Zool. Anzeig.* xxii. 1899, pp. 241-246. Prof. Sixta has been informed by Alois Topič, who lived for many years in Australia, that the mother duckmole lies down on her back, and that the two young ones press the milk out through the sieve-like apertures with their bills. The milk flows into a median groove which is formed by the longitudinal muscles. Until they are 12 cms. in length the young remain in the nest; when they measure 20 cms. they are taken by the mother into the water.

SMELL IN BIRDS. XAVIER RASPAIL. "Le sens de l'odorat chez les oiseaux." *Bull. Soc. Zool. France*, xxiv. 1899, pp. 92-102. It is a common statement that while nocturnal birds have a fine sense of smell, the diurnal birds of prey are guided solely by sight. Indeed, in many good zoological works, the sense of smell in birds is said to be almost nil. Against this, Raspail protests vigorously, and cites his observations on rooks, magpies, and blackbirds, which

seem to show that the sense of smell is well developed. He goes the length of saying that birds are endowed with the sense of smell at least equal to that of the dog.

NUCLEI OF MAMMALIAN RED BLOOD CORPUSCLES. A. NEGRI. "Ueber die Persistenz des Kernes in den roten Blutkörperchen erwachsener Säugethiere," *Anat. Anzeig.* xvi. 1899, pp. 33-38. The student who in his practical examination identifies distinctly nucleated red blood corpuscles as mammalian does not win favour in the eyes of the examiner, and this is perhaps well. But Mr. A. Negri, stud. med., has shown that there is still relevancy in inquiring into the possible persistence of the nucleus in the red blood corpuscles of adult mammals. The persistence of a nucleus has been asserted repeatedly, and, we believe, always given up. Perhaps only Petrone has stood to his guns and maintained *contra mundum* that to say the nucleus is absent is to confess ignorance of the proper method for its discovery. Negri has worked with Petrone's method, but finds that Petrone's "nucleus" is to be found in the embryo along with, but distinct from, the nucleus which is still evident in the red blood corpuscles in intrauterine life.

URNS OF SIPUNCULUS. S. J. METALNIKOFF. "Das Blut und die Excretions-organe von Sipunculus nudus," *MT. Zool. Stat. Neapel*, xiii. 1899, pp. 440-447. The strange multicellular ciliated bodies which occur in the body cavity and blood of Sipunculids have been much discussed and variously interpreted. According to Metalnikoff, they arise, in part at least, on the internal walls of the blood vessels, and serve to protect the animal from the ill-effects of hard particles which may be ruptured from the gut into the body cavity. The suggestion of Cuénot and others that the urns by their rapid movements help to compensate for the absence of a heart is also accepted.

BEETLES IN SELF-DEFENCE. L. BORDAS. "Les glandes défensives ou glandes anales des Coléoptères," *Ann. Fac. Sci. Marseille*, ix. Fasc. v. pp. 1-45, 2 pls. In this memoir, which our French colleague has been kind enough to send us, it is shown that the majority of beetles (Cicindelidae, Carabinae, Harpalinae, Feroniinae, Brachininae, Dytiscidae, Gyrinidae, Staphylinidae, Silphidae, etc.) possess in the posterior abdominal region a pair of glands, disposed in a cluster or in a tube, producing a secretion which is forcibly ejected in self-defence. These anal or defensive glands belong to the last abdominal segment, and consist of a glandular portion, an efferent canal, a reservoir or receptacle, and an excretory duct.

DEVONIAN ROCKS OF ARCTIC EUROPE. TH. TSCHERNYSCHEW and N. JAKOWLEW. "Die Kalksteinfauna des Cap Grebeni auf der Waigatsch-Insel und des Flusses Nechwatowa auf Nowaja-Semlja," *Verhandl. Russ. Kais. Mineral. Ges.* xxxvi. pp. 55-99, pls. vi.-viii. 1899. Many authors have written much on the Palaeozoic rocks and fossils of Waigatsch and Nova Zembla, but their statements have lacked precision, their conclusions definiteness. Two horizons are here determined in Waigatsch. The one, containing *Spirifer waigatschensis*, n. sp. and five other brachiopods, is paralleled with the upper limestones of the Middle Devonian in the Ural, containing *Spirifer anossofi* and *Stringocephalus burtini*. The other, furnishing *Proetus waigatschensis*, *Lichas (Dicranogmus) lindströmi*, *Leptodomus borealis*, *Spirifer parvulus*, n. spp., appears equivalent to the limestone of Nova Zembla, which contains *Cardiola lehmanni*, n. sp. Other fossils, such as *Orthoceras cinctum*, *O. cf. tentaculare*, *Whitfieldella didyma*, *Leperditia nordenskioldi*, show that this is not older than Middle Devonian.

SOME NEW BOOKS.

EAST AFRICAN SPORT.

Sport in East Central Africa, being an account of Hunting Trips in Portuguese and other districts of East Central Africa. By F. VAUGHAN KIRBY. 8vo, pp. xvi. + 340, with 4 plates. London: Rowland Ward, Limited, 1899. Price 8s. 6d.

Mr. Kirby is already known to the sporting world as the author of "In Haunts of Wild Game"; and the interesting experiences narrated in the latter work naturally lead the reader to expect as many exciting adventures in the new venture. In this matter it may confidently be said that expectation will not be disappointed; the adventures which befell the intrepid author in his pursuit of lions, elephants, hippopotami, and rhinoceros being little short of marvellous, although all bearing the mark of truth. The greater part of the country traversed by Mr. Kirby lies in the provinces of British Central Africa and Portuguese East Africa, and those who follow in his footsteps will doubtless benefit much by the descriptions given of the different routes. It would, however, have been a decided advantage if the publishers could have seen their way to issue an explanatory map, but the price at which the book is sold probably rendered this impossible. In his first work the author showed a tendency to write unduly long and complex sentences; and we are glad to notice an improvement in this respect in the present volume, although in some cases a still further curtailment, both as regards length of sentences and general redundancy of expression, would be desirable.

Much of the volume is taken up by the ordinary routine of marching and camp-life; but in the second half the real sporting adventures are so thickly crowded that almost every page is of thrilling interest. In this part of the Dark Continent at any rate, unless the rinderpest has subsequently done its fell work of destruction, the game is evidently not yet on the verge of extermination.

But Mr. Kirby is something more than the ordinary sportsman, and displays a keen interest in Natural History. This is exemplified by the well written appendix, in which all the larger species of mammals met with during the trip are recorded, with notes on their distribution and habits. In one respect the author displays a curious ignorance, this being his failure to grasp the meaning of the term "type" in Zoology. For instance, on page 338, he falls foul of the editor of the "Royal Natural History" for calling the original white-legged variety of Burchell's zebra the typical form, on account of its not being the one met with commonly at the present day! Of course the editor of the "Royal Natural History" is perfectly right, and his would-be critic, hopelessly wrong.

To those interested in a comparatively little known portion of Africa, Mr. Kirby's volume may be cordially commended, and we may at the same time call attention to the very valuable series of works on African sport and natural history now in course of publication by Mr. Rowland Ward.

THE BRAINS OF MAMMALS.

Handbuch der Anatomie und vergleichenden Anatomie der Centralnerven-Systems der Säugethiere: I. Makroscopischer Theil. By Drs. E. FLATAU and S. JACOBSOHN. 8vo, pp. xvi. + 578, with 7 plates and 126 figs. Berlin: S. Karger, 1899. Price 22 marks.

Perhaps we can bestow no greater praise on this elaborate and bulky treatise (which, by the way, only forms a first instalment of the complete work) than the expression of the wish that it may be found possible to republish it on a reduced scale in English. We say in an abbreviated form on purpose, because in these high-pressure times there is scarcely any one save the specialist who can afford time to wade through the mass of detail brought together by the learned author; and it is important that students of mammals, other than brain-specialists, should make themselves acquainted with the leading facts of the present line of investigation. Although, so far as we are aware, there is no work in English specially devoted to the central nervous system of mammals, we are glad to see the authors of the volume before us confessing their indebtedness to British investigators like Cunningham, Beddard, and Garrod.

The plan adopted by the authors is to take leading representatives of the various mammalian orders in regular sequence and to describe in detail the brain-characters in each, more space being naturally devoted to the complicated brain of the Chimpanzee than is assigned to its simpler representative in the Duckbill or Echidna. One method of illustration that especially commends itself to us is the delineation of the position of the chief cerebral sulci on the outer surface of the skull of the animal to which the brain in question pertains. By this means an excellent idea is gained not only of the relative proportion of the brain to the skull, but also as to the relative complexity of brain-convolution in different animals. At the close of the work are given the general results of the authors' investigations; and some very interesting facts are recorded as to the relation of the volume of the brain to that of the skull, the absolute brain-weight, and the relation of the latter to the corporeal weight. Needless to say that these investigations tend in no wise to a revival of the cerebral classification of Mammals attempted by Owen.

In only one respect have we to find fault with the authors, and this relates to the names employed for some of the animals treated of. It is a well-known complaint on the part of systematists that anatomical and physiological writers are generally remiss in regard to nomenclature, but it is seldom that we encounter such a gross anachronism as the retention of the name *Simia troglodytes* for the Chimpanzee. Several minor errors in nomenclature also occur. And here it is desirable to warn the advocates of radical changes in mammalian nomenclature that such are scarcely ever adopted by non-systematists (who probably never see them), so that instead of promoting uniformity, which is the only justifiable plea for their introduction, such changes in names only lead to worse confusion than ever. The volume closes with a comprehensive list of literature, in regard to which it may be remarked that it is a pity some person with a better knowledge of English than is apparently possessed by the authors was not asked to read the proof-sheets.

The work, when complete, will doubtless long remain the standard authority on the interesting but difficult subject of which it treats.

"OUTLINES."

Outlines of Zoology. By J. ARTHUR THOMSON, M.A. Third Edition, Revised and Enlarged. 8vo. pp. 819, with 332 illustrations. Edinburgh & London: Young J. Pentland, 1899. Price 15s.

Professor J. Arthur Thomson is to be heartily congratulated on the issue of the third edition of this well-known text-book. In the space of 819 pages the author touches upon almost every side of zoological science. As the title of the work explains it is simply "Outlines," and although there is always a danger in treating of the multiplicity of subjects herein contained, we are forcibly impressed with the freshness and clearness with which they are presented.

This is the only zoological text-book in the English language which aims at a complete review of zoological science, and the best evidence that such a work was wanted and is appreciated by teachers and students of zoology, is supplied by the issue of the present edition.

The correlation of structure and function which is emphasised throughout the work is an admirable feature, as also the "up-to-dateness" which cannot fail to stimulate the student.

Many new figures have been added and some corrected. While the revision of the illustrations was taking place it is a pity that some of those which have done duty for so long have not been eliminated, such for instance are Fig. 73 representing the proglottis of a Cestode ("Constructed from Leuckart") in which the nervous system is omitted, Fig. 83 of the reproductive organs of *Lumbricus* (after Hering) in which the ovaries are incorrectly figured, Fig. 150 a "dissection of *Helix pomatia* (mainly after Leuckart") in which the position of the heart is wrongly shown. It is questionable if figures 199 and 215 are worth the space they occupy, while Figs. 234 and 235, representing the urino-genital organs of the male and female frog, would undoubtedly have been more useful if of *Rana temporaria* rather than *R. esculenta.*

In a fourth edition we should like to see the confused account of the renal and reproductive organs of the skate (pp. 496-497) re-written, and the terms Wolffian and Müllerian ducts omitted.

A word must be said in praise of the tabular form of summaries of affinities, etc., in chapter xx., as indeed of those throughout the work, all of which are admirable.

This delightfully written text-book has enjoyed an enviable reputation in the past, and the present edition can only enhance the same.

WALTER E. COLLINGE.

PRACTICAL ZOOLOGY.

Leitfaden für das Zoologische Praktikum. By Dr. WILLY KÜKENTHAL, Professor in Jena. 8vo, pp. vi. + 284, with 172 text-figures. Jena: G. Fischer, 1898. Price, sewn 6 marks, bound 7 marks.

This is intended as a guide for beginners, whether in a properly appointed laboratory or working independently. For the latter there are given many technical instructions, for the lack of which the elementary student so often finds himself at sea. The opening chapter is on apparatus and the way to use it, and contains many useful hints. Thus the author rightly insists on the necessity for drawing on a large scale—"Don't spare paper, but take a fresh page to each drawing." Then follows a chapter on the elements of histology, in which, after an illustrated summary of the various tissues, it is shown how they may be demonstrated. The student is then led through nine phyla of the animal kingdom, beginning with Protozoa and ending with Vertebrata. Each of these is preceded by a systematic synopsis, enabling the student to ascertain

the position of the species under investigation, but of course not intended to supplant the ordinary text-book of systematic zoology. The study is divided into twenty lessons, and at the beginning of each is a short statement of the material and reagents required, followed by a general account of the Class or Order. The directions for the actual dissection and demonstration are clear and straightforward, and are elucidated by a number of figures. Of these illustrations many are original, and due either to the author or to his pupils, Messrs. Th. Krumbach and A. Giltsch. Others are borrowed, and we are glad to note that the original source is given with accuracy; but is not "Fig. 95. Organisation von *Holothuria tubulosa* (aus Lang)" really copied from Milne Edwards and Carus? The drawings are good, they will help to sell the book, and the beginner will be grateful for them. None the less, they may tempt the student to adopt the easier course of lifting them into his note-book instead of drawing from the object before him. And is it not a good training for the student to direct him to the original monographs, and to let him copy the figures (if he does it at all) from the first source of each? There is little in this book to lead the student on, or to disabuse him of the notion that, when he has worked through what is here, he will have as thorough acquaintance with the various types as is needful. The course is professedly an elementary one, and little attention is paid to other methods than those of dissection with scalpel and needle. But even so, it is startling to find *Sepia* taken as the type of a Cephalopod, and yet no description given of the cuttle-bone.

There are so many good books of the kind nowadays, that this one by Professor Kükenthal is not likely to find a large sale outside Germany, even if translated. But it can be recommended as accurate, clear, and adapted to the somewhat narrow limits of an elementary course.

F. A. B.

MONTH BY MONTH.

Rambles with Nature Students. By ELIZA BRIGHTWEN, F.E.S. Pp. 221, with many illustrations. London: Religious Tract Society, 1899.

Mrs. Brightwen has published another of her delightful little books of talk about common things. The present volume contains six or seven short chapters for each month, and with just a little help from the treasures of her museum in the barest months the authoress contrives to find interesting subjects throughout the year. In the dull days she gives us pretty and well-illustrated studies of ice-crystals, footprints in the snow, skeleton leaves, birds' feet and skulls, ventriculites, and various other matters. During the brighter months she writes simply and clearly of many familiar insects and flowers, and of some, too, like those in her chapter on "Hidden Lives," that are known only to those whose eyes have been trained to see. Her descriptions are always vivid and interesting, and the practical directions frequently given are clear and simple. Her new book will prove not only helpful and stimulating to those who have already done some work for themselves, but will also be a most comforting guide for such easily-discouraged little people as the twelve-year-old, who abandoned the study of natural history because, as she plaintively said, the beasts never had any habits when she was watching them.

The naturalist's delight in living things for their own sake by no means obscures Mrs. Brightwen's keen appreciation of their practical aspects. Thus we may learn from her chapter on the development of flies what precautions should be taken to protect our meat from bluebottles, from the life-story of the meal-worm how to keep up an unfailing supply of animal food for our cage birds, and she tells us, too, that a tonic beverage may be made from acorn-kernels, and that she was able to express from a fungus, the "maned agaric," a serviceable ink whose qualities were unimpaired after eleven years. The ingenious way in which, by a process of pith-slicing and repeated ironing, she

succeeded in making, from a papyrus in her hothouse, a paper exactly resembling the ancient parchments of the East, commands our highest admiration. But what shall we say of a green satin banner-screen, embroidered with jasmine sprays, of which the starry flowers were simulated by five otoliths of fishes, and the leaves by rose-beetle wings? M. R. T.

A STRANGE MIXTURE.

The Philosophy of Memory; and other Essays. By D. T. SMITH, M.D., Lecturer on Medical Jurisprudence in the University of Louisville. 8vo, pp. 203. Louisville, Ky.: John D. Morton and Co., 1899. Price $1·25.

This work is a collection of essays upon very diverse subjects. How wide is the range a mention of the different titles will indicate. Besides the essay on the Philosophy of Memory, which gives its name to the book, there are articles on the Functions of the Fluid Wedge, the Birth of a Planet, and the Laws of River Flow.

The degree of mental equipment which the author possesses, and the measure of intelligence which he brings to bear upon these subjects may, perhaps, be illustrated in the following manner:—After some 70 pages of argument concerning memory, the author says, "Every animal in every part, every leaf in its pattern of shapeliness," etc., etc., etc., "is now built up and developed by the forces of nature playing on it chiefly from the worlds beyond. It is the little waves of ether, coming mostly from the sun, that build up the plant, and by their ceaseless pelting drive every atom and every molecule to its place" (p. 78). And "The tenderest feelings must have a higher origin . . . than that of the familiar forms of force; and nothing appears as their proximate source except the fading undulations of light as they journey through infinite space—the 'sweet influences of the Pleiades'" (p. 80).

In the essay on the Birth of a Planet the author brings forward several, at any rate plausible, arguments against the nebular theory; but then he concludes, "One might be tempted to suggest . . . that worlds have a season to bring forth, as do animals and plants, and that in their proper times and seasons, fixed in the infinite councils, they drop their ripened fruit of young worlds into space" (p. 136). We are tempted to suggest, knowing the universal solicitude of the British Parliament for all afflicted, that the new Midwives Bill provides for the case of a world in labour. We cannot afford to lose a world through the ministrations even of a celestial Sairey Gamp.

The essay on the Laws of River Flow suffers from association. The author does not suggest "light from the Pleiades," or the "infinite councils" having any controlling influence on river flow. He leaves a volume of water to its own devices, and suggests that it moves, in flowing, "like two equal cylinders revolving spirally on parallel axes in different directions, outward at the bottom, upward at the margins, inward at the top, and downward through the middle."

The movements of a body of water flowing along a channel are evidently most complicated. Whether among other movements it has that which the author suggests might be determined in the laboratory. It should not be difficult to devise a series of experiments adequate for the end in view.

S. S. B.

AN ALPINE GUIDE.

Hints and Notes for Travellers in the Alps. By the late JOHN BALL. A new edition by W. A. B. COOLIDGE. 12mo, 164 pp. London: Longmans, Green & Co., 1899. Price 3s.

The late Mr. John Ball's "Hints and Notes," forming the General Introduction to his "Alpine Guide," is too well known and too highly appreciated

by all visitors to Switzerland, to need more than a reference to the new matter introduced into this edition, which is both interesting and important. The chapter on the geology of the Alps has been practically rewritten by Professor Bonney, and that on the climate and vegetation of the Alps has been expanded by Mr. Percy Groom. In addition to this, Mr. Sydney Spencer adds a new chapter on photography in the High Alps; and the editor contributes one on Life in an Alpine Valley, and an exceedingly useful Glossary of alpine terms.

It will be seen, therefore, that the volume forms a complete *vade mecum* for visitors to the Alps, whether climbers or ordinary tourists, its small and compact size fitting it admirably for the pocket or the knapsack.

The chapter on "Life in an Alpine Valley," should be read by everyone who cares to know anything about the social condition of the people among whom he is travelling. It treats of the daily manner of life of the dwellers in the mountain valleys, the customs regarding the ownership of landed property, the rights of use of the "Alps," and other details. The limitation which the editor himself lays down should, however, be borne in mind by the reader, that his description applies mainly to that portion of Switzerland with which Mr. Coolidge's residence at Grindelwald has made him specially acquainted. Thus the statement that "spinning and weaving have almost disappeared" from the mountain châlets does not apply to the Ausser Rhoden of Canton Appenzell.

A. W. B.

LIQUID GASES.

Liquid Air and the Liquefaction of Gases: Theory, History, Biography, Practical Application, Manufacture. By T. O'Conor Sloane, Ph.D. 8vo, 365 pp., with illustrations. London: Sampson Low, Marston, & Co.

This little book gives a readable account of the work done on the liquefaction of gases, which has of late met with so much success, and has attracted so much popular attention. The author begins with a short exposition of the facts and scientific principles underlying the obvious phenomena of change of physical state, and describes the various appliances necessary for the measurement of very low temperatures. In succeeding chapters he shows the historical development of the subject, beginning with the foundation of the Royal Institution in 1799, reviewing briefly the early work of Northmore and Faraday, describing in greater detail the life and labours of Pictet and Cailletet, finally to deal with the "moderns" Dewar, Tripler, Linde, and Hampson. The biographical notices are interesting, and many of them are accompanied by good portraits. Chapters on experiments with liquid air and on the practical applications of very low temperatures conclude the volume. It is gratifying to learn that the author in no way countenances the absurdly exaggerated accounts that have appeared recently in many newspapers regarding liquid air as a source of energy. While he says (p. 356), "Liquid air, if it could only be produced cheap enough, would represent an ideal substance for the production of energy," he has carefully stated on a previous page (p. 72) "The trouble is that to produce liquid air we have hitherto been obliged to expend a great deal more available energy than we can utilise of normally unavailable energy by its gasification."

Ch.

HISTORY OF CHEMISTRY.

A Short History of the Progress of Scientific Chemistry in our own Times. By William A. Tilden, F.R.S. 8vo, x. + 276 pp. London: Longmans, Green, & Co., 1899. Price 5s.

Professor Tilden in his preface says, "In the following pages I have endeavoured to provide for the student such information as will enable him to

understand clearly how the system of chemistry, as it now is, arose out of the previous order of things; and for the general reader, who is not a systematic student, but who possesses a slight acquaintance with the elementary facts of the subject, a survey of the progress of chemistry as a branch of science during the period covered by the lives of those chemists who were young when Queen Victoria came to the throne." This self-imposed task has been admirably accomplished. In brief compass he sets before the reader an easy account of the most striking facts and theories of modern chemistry in their origins and in their final development. Thermochemistry, spectrum analysis, the periodic system of the elements, the synthetic production of dyes, drugs, and explosives, stereochemistry, and the action of ferments, all receive simple and adequate treatment. To both student and general reader the book can be warmly recommended. CH.

A MUSEUM HANDBOOK.

The Manchester Museum, Owens College. General Guide to the Natural History Collections. By W. E. HOYLE. 8vo. pp. 78. Manchester Museum, Publication 28, 1899. Price 6d.

Distinctly a Museum Handbook, in that it guides the visitor, gently but firmly, through the museum from case to case, from minerals and geological phenomena, through the array of fossils stratigraphically disposed, then along the animal collections in the order of their arrangement (not always harmonious with the text-book), and finally through the botanical exhibits. Those who wish for a cut-and-dried classification will find in the form of appendices: "A. List of the principal divisions of the Earth's Crust;" "B. List of the principal divisions of the Animal Kingdom," with a typical example of each class mentioned in the vulgar tongue; and C. the same for the Vegetable Kingdom. In the monstrously difficult task of writing in simple language an accurate and not uninteresting summary of the Animal Kingdom Mr. Hoyle has achieved as much success as is possible. All the same, why does Mr. Hoyle say (p. 8) that the Devonian Crinoids "were of the type known as Cystids"? The division of the Crinoidea generally (p. 56) into "sea-lilies" and "feather-stars" is due of course to the two volumes of the Challenger Report. It is a book-binder's classification. The account of the Geological divisions is as good as one could hope to find in a score of pages. But the two pages devoted to the Mineralogical and Petrological Collection ought to be multiplied by at least ten, or else omitted. It is a pity they should form an opening to the Guide. The compression of the guide to the Botanical Collection into seven pages may have been enforced; if so, it is to that cause we will charitably ascribe the appearance of such unexplained terms as "saprophytic," "prothallium," "carpellary," "dichotomous," and the sweet little "bulbils." These fancy words are not in the picture with the rest of this excellent handbook. F. A. B.

THE NOTES OF BIRDS

The Cries and Call-Notes of Wild Birds. A popular Description of the Notes employed by our commoner British Birds in their Songs and Calls. With Musical Illustrations. By C. A. WITCHELL, Author of "Evolution of Bird Song," etc. 8vo, pp. xi. + 84. London: L. Upcott Gill, 1899. Price 1s.

One of the greatest charms of field ornithology is supplied by the various cries and songs uttered by different groups and species of birds. Much attention has been devoted to this subject by our continental *confrères*, some of whom have excelled in their skill in rendering upon paper the love-notes and

alarm-cries of bird-colonies. In the present case, an English ornithologist furnishes an interesting collection of his own rendering of bird-notes. Probably no two persons would express the more difficult notes in exactly the same way, but an approximation to truth is by no means impossible. Mr. Witchell has devoted so much loving labour to the study of his favourite subject, that many people besides professed naturalists will welcome the present volume, and find that it stimulates their endeavours to acquaint themselves with all the different notes that enliven our shores and forest haunts. The treatise is popularly written, and the songs of a good many birds are expressed in musical notation.

H. A. Macpherson.

The latest number of the *Transactions of the British Mycological Society* contains a summary of the Fungus Foray held at Dublin in September 1898, and the papers read at the meetings. The Foray must have been conducted with energy, for 160 species were added to an already existing list of 830 species for the counties of Dublin and Wicklow. In the report useful references to suitable neighbourhoods and to the local literature will be found. Among the more important papers are those by Dr. C. B. Plowright, who acted as president of the meeting. His address on the Agaricini, and a contribution on "New and rare British fungi," are useful and practical. A summary of the recent work of Eriksson, of Stockholm, on the Uredineae of cereal crops is particularly valuable, because, during the past year, that author has given articles on the same subject to almost every existing botanical magazine, till he has landed the student in a hopeless maze of references; a clear summary like this one was much needed. The Dublin members, Mr. Greenwood Pim and Dr. M'Weeney, have contributed useful papers, the latter throwing light on two sclerotium diseases of the potato. Two papers in the number before us are merely reprints of the British Association reports of the 1898 meeting; they are both rudimentary notes on laboratory work done at Cambridge, and it seems absurd that such should be presented in the same month to the British Association and again to the Mycological Society; still more superfluous that one should meet them here for at least the fourth time in the literature of botany. Dr. Plowright gives obituary notices on two eminent fungologists—Rev. Canon Du Port and Mr. H. T. Soppitt, with good portraits.

We have received the first number of the *Polyclinic*, being the journal of the Medical Graduates' College, London, a journal which does not at first sight much concern readers of *Natural Science*, however strongly they may in other capacities sympathise with the aims of this admirable institution. Yet as we turn over the pages with a biological eye, we feel impressed by the fact that while knowledge is manifold there is only one science. Sir William Broadbent, with the progress of science for his keynote, Mr. Jonathan Hutchinson, with the motto, "'Tis the taught already that profits by teaching," Dr. Miller Ord, with the proverb "Docendo discimus," expound the aims of the college; and as we pass to courses of lectures we see "functions of the nervous system," "family history in nervous disease," "diseases of animals," "experimental teratogeny," "dissolution of heredity," "physiology of germinal life," and much more, which shows that the journal has much common ground with ours. Floreat.

The June number of the *Journal of School Geography* contains, *inter alia*, articles on Southern California, by Mr. J. F. Chamberlain; on the geographical and geological exhibition at Springfield, Mass., by Professor R. E. Dodge; on pressure, winds, and rainfall over the British Islands by Dr. A. J. Herbertson. Among the exhibits referred to are the great relief map of the United States, showing the curvature of the globe, and with the glacial ice-cap, two relief globes, the Spruner-Bretschneider charts, illustrating the development of Europe from 350 A.D. to the close of the Napoleonic wars, the series of 37 Charakter-

Bilder, by Hölzel of Vienna, the forestry maps of Sargent, showing the distribution of trees in North America, and many other items of importance which suggest, like the journal itself, the great progress at present being made in the science and teaching of geography.

In *Science* of June 23 Prof. R. W. Wood describes his diffraction process of colour-photography, which is really a variation of the three-colour method; and Prof. E. Thorndike discusses the mental fatigue of school work, furnishing additional data which render more probable his previous conclusion that "the mental work of the school day does not" [at the time] "produce any decrease in the ability to do mental work."

The eye of the Amphipod Crustacean *Biblis serrata* receives attention at the hands of Dr. S. D. Judd in the May issue of the *Proc. Biol. Soc. Washington*, and is found to be remarkably different from the corresponding organ of *Gammarus*. It appears to be a compound eye constructed on the general plan of an ocellus, but furnished with a space which may be the functional representative of the space occupied by the vitreous humour in the vertebrate eye. Further investigations are, however, needed before the full significance of all parts of this organ can be determined.

The Alaskan Moose, or Elk, has long been known to be the largest representative of its kind, and it appears to be mainly on this feature that Mr. G. S. Miller relies in describing it as a new species (*Alces gigas*) in the recent issue of the serial last quoted. Most English writers regard all the living representatives of the Elk as referable to a single wide-spread species. In recognising three specific forms in what is essentially one and the same animal, Mr. Miller shows the value to be attached to species recently named by American writers among the smaller Mammals.

Prof. Weismann's essay on regeneration appeared contemporaneously in *Natural Science* (in English) and in the *Anatomischer Anzeiger* (in German). A reprint of the German edition has been published in pamphlet form by Mr. Fischer of Jena, to whom we are indebted for a copy. It is entitled "Thatsachen und Auslegungen in Bezug auf Regeneration," occupies 31 pages, and costs 60 pfennigs.

OBITUARY.

SIR W. H. FLOWER, K.C.B. (1831-1899).

It is with sincere regret that we have to record the death of Sir William Henry Flower, which took place at his residence in Stanhope Gardens, on the afternoon of Saturday, July 1, after a protracted period of failing health. It was owing to this ill-health that he resigned, in August last, the Directorship of the Natural History Branch of the British Museum; and although a residence during the past winter in the Riviera led to a temporary improvement, on his return to Stanhope Gardens in May it was but too evident that no permanent benefit had taken place in his condition, and that the end could not be far distant. After a short rally, a serious relapse occurred on the Thursday preceding his demise, which resulted in a fatal attack of pneumonia.

Sir William was the second son of the late Edward Fordham Flower, of Stratford-upon-Avon, Warwickshire, by his wife, Celina, daughter of the late John Greaves, of Radford, Warwickshire, and was born on November 30, 1831, at his father's residence, The Hill, Stratford-upon-Avon. The latter part of his education was conducted at University College, London, where he went through the ordinary course of medical study, eventually qualifying as a surgeon. We believe we are right in saying that the career of an army-surgeon was not his original intention, but that the need of additional surgeons for the army induced him to volunteer at the outbreak of the war for service in the Crimea. At any rate, he was at that time attached, in the capacity of assistant-surgeon, to the 63rd regiment, with which he served throughout the long campaign, receiving at its close the Crimean medal, with the Alma, Inkerman, Balaclava, and Sebastopol clasps, and also the Turkish medal. With the close of the war his services as an army-surgeon also came to an end; and after his return to England he was appointed in 1859 Assistant-Surgeon and Demonstrator in Anatomy at the Middlesex Hospital. Mr. Flower (as he then was) did not, however, long retain this post, which he vacated in 1861 to take up the more congenial duties of Conservator of the Museum of the Royal College of Surgeons, a position which he occupied till his transference to the British Museum in 1884. In the meantime (1870) he was, however, chosen to succeed Owen as Hunterian Professor of Comparative Anatomy and Physiology to the College—a post which he likewise held till the severance of his official connection with the College. The resignation in 1884 of Sir Richard Owen caused the Directorship of the Natural History Branch of the British Museum to become vacant; and to this important position Professor Flower was shortly afterwards appointed. During his tenure of the Directorship, he was successively gazetted C.B. in 1887, and K.C.B. in 1892. In the ordinary course of events, Sir William's connection with the Museum would have terminated on his attaining the age of sixty-five in 1896. But, on the earnest recommendation of the Trustees, the Treasury was induced to waive the age-disqualification in his case; and it was during this unexpired period of extension of service that Sir William was compelled by ill-health to tender his resignation.

In addition to the distinctions conferred by his Sovereign, Sir William Flower was the recipient of numerous other honours from academic and scientific bodies. In 1864, he was elected to the Fellowship of the Royal Society, from whom, in company with Lord Rayleigh, he received the award of a Royal medal in 1882. He served on the Council of the same Society for three separate periods, namely 1868-1870, 1876-1878, and 1884-1886; and from 1884 to 1885 filled the office of a vice-president. He was a Fellow of the Royal College of Surgeons of London. The degrees of D.C.L. and LL.D. were

conferred upon him respectively by the Universities of Oxford and Cambridge; and he was also the recipient of those of D.Sc. and Ph.D. So far back as 1851 he became a Fellow of the Zoological Society, of which body he was elected president in 1879—an office he held at the time of his death. From 1883 till 1885, Sir William also occupied the presidential chair of the Anthropological Institute; while in 1887 he served in the same capacity at the meeting of the British Association, having presided over the section of Biology at the meeting of 1877, and that of Anthropology in 1881. He was also President of the section of Anatomy at the International Medical Congress at its London meeting in 1881; and it was solely due to ill-health that he was prevented from presiding over the International Congress of Zoology held last year at Cambridge. Both the Geological and the Linnean Societies of London claimed Sir William as a Fellow.

As examples of his devotion to his own work, it may be mentioned that it is within the knowledge of the present writer, that Sir William refused both the Presidency of the Royal Society, and a seat in the Senatus of London University (in succession to Huxley), on the ground that they would interfere with his official duties.

From his very earliest days Sir William Flower displayed a marked love and inclination towards natural history studies; and in his last work, "Essays on Museums" (which is a collection of articles compiled while incapacitated by illness from more severe labours), he takes the public into his confidence to tell them how he first began collecting and arranging zoological specimens in early boyhood. With his appointment to the Museum of the College of Surgeons, opportunities for cultivating that branch of zoological science he loved best, namely, the anatomy and classification of mammals (inclusive of man), were abundant, and good use was made of them. Nearly every portion of the osteological collection of the College still bears the impress of his work; the series of human skulls and skeletons having been vastly increased during his tenure of office.

A permanent record of his zeal in augmenting and classifying the Hunterian collection is afforded by the two volumes of "Catalogues" compiled by him, with the assistance of Dr. Garson, during his tenure of office; one of these, published in 1879, being devoted to the osteology of man, while the second (1884) treats of that of other mammals.

During his tenure of the Hunterian chair, Professor Flower regularly delivered the annual course of lectures; the substance of the first series of these being expanded into the now well-known "Introduction to the Osteology of the Mammalia," the first edition of which appeared in 1870, and the third (revised with the assistance of Dr. H. Gadow) in 1885.

For several years after his appointment to the British Museum, Sir William's attention (in addition to the routine work of his office) was largely occupied with the formation and arrangement of the "Index Museum," which now occupies the bays on the sides of the central hall; while he was also engaged with the acquisition and mounting of the interesting specimens exhibited in the cases standing in the hall itself. When, however, the office of Keeper of the Zoological Department was held by him conjointly with the Directorship, Sir William in due course determined to rearrange at least the Vertebrate Galleries of the Museum according to his own ideas—a work which is still in progress. As is well known, it was his idea that no specimens should be exhibited in a Museum to the public which do not actually teach something; and he was above all urgent as to the necessity of explanatory labels, which he regarded as of almost more importance than the specimens themselves. The results of his plan are now exhibited in the Mammal and Bird Galleries.

Although a diligent student of the structure of mammals belonging to all orders, Sir William's special favourites were undoubtedly man on the one hand and whales and dolphins on the other. And his last efforts during his tenure

of the Directorship were devoted to the completion of the life-size series of models of the latter animals, which now form such an attractive feature of the Museum, and also to the formation of an anthropological gallery which should worthily head the zoological series of the museum. Fortunately, he was enabled to witness the opening of the new whale gallery, which took place on Whit Monday of last year; but the comparatively advanced stage now reached by the anthropological series has been the work of other hands in the enforced absence of the originator.

With regard to the general scope and importance of Sir William Flower's scientific work, it is perhaps too early to form an exact opinion. The anatomy, classification, and distribution of the Mammalia undoubtedly formed his favourite themes; and it is largely to his influence and writings that our conceptions of the mutual relations of the different members of the class are due. Of course he was not infallible, as the present views as to the relationship of the marsupials to other mammals alone sufficiently attest. But he was remarkable for his devotion to accuracy; and the pains he would devote to the elucidation of small obscure points are well worthy the imitation of many of his more impetuous followers. Although no grand discovery or great generalisation is associated with the name of Flower, the amount of solid zoological work he has done, and, above all, the revolution which he has brought about in our conceptions of what a museum should be, cannot fail to have a marked influence on his successors for many years to come. We have not yet noticed that, in addition to being a zoologist, Sir William was also a most competent palaeontologist. And yet to him such a disassociation of ideas as these terms imply would have been in the highest degree repugnant, for it was a dominant idea of his that palaeontology is but the zoology of the past, and that the two subjects should be treated as one. This combination of palaeontological and zoological knowledge gave him a far wider conception of the relations of the various groups of the animal kingdom than is held by many of his contemporaries; and, although the force of circumstances prevented its accomplishment, it was his earnest desire to see, so far as practicable, the amalgamation of the recent and extinct specimens exhibited to the public in the great institution confided to his charge.

Although the number of scientific memoirs which stand in his name is very large, Sir William Flower is known to the general public by comparatively few works. Allusion has been already made to the "Catalogues" of the Museum of the Royal College of Surgeons and to the "Osteology of the Mammalia." To the ninth edition of the "Encyclopaedia Britannica," Sir William contributed the important article "Mammalia," as well as a number of minor articles on various representatives of the same group. These articles, together with a few by other writers, were subsequently, with the aid of the present writer, collected and expanded, so as to take the form of a systematic treatise published under the title of "An Introduction to the Study of Mammals" (1891). Later on in the same year appeared a little volume on "The Horse," in the "Modern Science" series; while the above-mentioned "Essays on Museums and other Subjects connected with Natural History" was published, under saddening circumstances, only last year. To allude to any of the numerous memoirs on technical subjects is obviously impossible on this occasion. Although somewhat reserved, and, perhaps, even occasionally cold in manner, Sir William Flower was greatly esteemed and beloved by a large circle of friends, both scientific and otherwise. When once the thin veneer of reserve was penetrated, no man could be kinder; and the trouble and attention he would devote to all who claimed his assistance were almost inexhaustible. To the present writer (if he may be permitted to say so) the loss is a very real and a very personal one. His first recollection of Sir William was in the Cambridge Natural Science Tripos of 1871, when the candidate little thought that he would one day be asked to join the (apparently) stern examiner in writing a treatise on one of the subjects of examination.

R. LYDEKKER.

NEWS.

The following appointments have recently been made:—Captain W. de W. Abney, C.B., to be principal assistant secretary of the Science and Art Department; Dr. G. Agamennone, as director of the Geodynamic Observatory at Rocca di Papa, near Rome; Joseph Barrell, as instructor in geology in Lehigh University, South Bethlehem, Penn.; Miss Annie J. Barrows, as assistant in zoology at Smith College, U.S.A.; Dr. Tarleton H. Bean, as director of forestry and fisheries on the U.S. Commission to the Paris Exposition of 1900; Dr. C. Benda, privat docent in the University of Berlin, nominated professor; E. A. Bessey, to be assistant vegetable pathologist in the United States Department of Agriculture; Dr. J. Warwick Brown, as external examiner in zoology in the University of Aberdeen; Dr. E. Wace Carlier, as professor of physiology at Birmingham; J. F. Collins, curator of the herbarium in Brown University, U.S.A., to be instructor in botany there; John G. Coulter, as instructor in botany at Syracuse University; Ulric Dahlgren, to be assistant professor of histology in Princeton University; Dr. J. Dewitz, as resident assistant of the *Concilium Bibliographicum*, whose new address is 38 Eidmatt Strasse, Zürich; Dr. Oliver L. Fassig, as instructor in climatology in Johns Hopkins University; Dr. John Gifford, as assistant professor of forestry at Cornell; Ulysses S. Grant, as professor of geology in the North-Western University; Dr. A. J. Herbertson, as lecturer on physical geography at Oxford; Dr. Robert Tracy Jackson, assistant professor of palaeontology in Harvard; Dr. Bengst Jöhnsson, professor of botany at the Academy at Lund; Sir George W. Kekewich, to be secretary of the Science and Art Department in room of Sir J. F. D. Donelly retired; Dr. B. F. Kingsbury, as assistant professor in histology and embryology at Cornell; Dr. L. Lalry, as correspondent to the *Concilium Bibliographicum* of Zürich; Professor Malcolm Laurie, as external examiner in zoology in the University of Glasgow; Mr. F. R. Lillie, as professor of biology at Vassar College; Miss Florence M. Lyon, Ph.D., as assistant in botany at Smith College, U.S.A.; Dr. R. S. Macdougall, as lecturer on botany at the Heriot-Watt College, Edinburgh; Dr. Rudolf Martin, as professor extraordinarius of anthropology in Zürich; Dr. E. B. Matthews, advanced to the position of associate professor of petrography and mineralogy at Johns Hopkins University; Mr. E. A. Minchin, as professor of zoology at University College, London, in succession to Professor Weldon, now of Oxford; Dr. G. Poirault, to succeed Naudin as director of the botanical laboratory for higher instruction at the Villa Thuret, Antibes; Dr. Adalar Richter, professor extraordinarius of botany in the University of Klausenburg; Miss W. J. Robinson, as instructor in biology at Vassar College; Dr. Alfred Schaper, to be assistant professor of histology at the Harvard Medical School, Boston, Mass.; Dr. Frank Schlesinger, as an observer in the U.S. Coast and Geodetic Survey; W. E. D. Scott, curator of the ornithological collections of the Green School of Science in Princeton; Dr. G. B. Shattuck, advanced to the position of associate in physiographic geology at Johns Hopkins University; M. V. Slingerland, as assistant professor in entomology at Cornell; Dr. Streckelson, privat docent for geography in the University of Basel; Dr. F. Strong of Yale, to be president of the University of Oregon; Professor Ph. van Tieghem, to the chair of the biology of cultivated plants at the National Agronomic Institute, Paris; Dr. Tobler, privat docent for mineralogy in the University of Basel; Dr. R. von Wettstein, to be professor of botany in the University of Vienna; Dr. Gregg Wilson, as lecturer on biology at the Royal (Dick) Veterinary College, Edinburgh, and on zoology at the Heriot-Watt College, Edinburgh; J. B. Woodworth, as instructor in geology in Harvard University.

Mr. G. A. Stonier has been appointed specialist in mining under the Geological Survey of India. Mr. Stonier holds the De la Beche medal for mining at the Royal School of Mines, London, of which institution he is an associate. He has had a wide experience in New South Wales, where he was employed as geographical surveyor, and was for several years a member of the Government Prospecting Board.

Dr. Adolph Fick, professor of physiology in the University of Würzburg, has resigned at the age of 70 years.

The Royal Commissioners for the Exhibition of 1851 have approved the nomination by the University College of North Wales of Mr. Robert Duncombe Abell, B.Sc., to a Science Research Scholarship of the value of £150 a year. Mr. Abell is about to enter the University of Leipzig, where he proposes to engage in research under the direction of Professor Wislicenus.

Mr. James Muir, instructor in Agriculture to the Somerset County Council, has been awarded the prize of 500 guineas offered by the sulphate of ammonia committee for the best essay on the utility of this salt in agriculture.

The Isidore Geoffroy Saint Hilaire Grand Silver Medal of the Société nationale d'acclimatation de France, has been awarded to Prof. Cossar Ewart for his breeding experiments (in reference to which he has also received the Neill Prize from the Royal Society of Edinburgh), and to Miss Ormerod for her entomological work.

The University of the Cape of Good Hope has conferred the honorary degree of D.Sc. on Mr. Alexander W. Roberts, of Lovedale, who has interested himself in astronomical observations there.

Yale University has conferred the degree of LL.D. on Prof. C. S. Minot, and Hobart College on Prof. W. K. Brooks, two of the most outstanding representatives of biology in America.

Prof. H. A. Pilsbry, of the Philadelphia Academy of Natural Sciences, whose work on Mollusca is familiar to students, has received the degree of Doctor of Science from the University of Iowa.

The following have been elected foreign members of the Royal Society:— Prof. L. Boltzmann, of Vienna; Dr. Neumayer, of Hamburg Observatory; Dr. Anton Dohrn, of Naples; Prof. E. Fischer, of Berlin; and Dr. M. Treub, of Buitenzorg Botanical Gardens.

Sir W. T. Thiselton Dyer, K.C.M.G., F.R.S., has been elected to an honorary studentship at Christ Church, Oxford.

The first Nobel prizes in physics, chemistry, medicine, literature, and for the promotion of peace, each of the value of 15,000 kroner (about £2500), will be conferred in 1901, on the 18th December, on the anniversary of Nobel's death. Any one making application for one of the prizes is thereby excluded.

We are glad to learn that Mr. Oldfield Thomas has returned to the British Museum (Nat. Hist.) completely restored to health.

Prof. Angelo Mosso has gone to America to deliver a lecture on the "Psychic Processes and Movement" at the anniversary celebrations at Clark University, Worcester, Mass.

Prof. W. C. Brögger, of the University of Christiania, has accepted an invitation to deliver the second course of the George Huntington Williams memorial lectures at the Johns Hopkins University in April 1900. Prof. Brögger is the most prominent Scandinavian geologist, and is well known for his memoirs upon the geology of Southern Norway. He will lecture upon modern deductions regarding the origin of igneous rocks. The first course was given two years ago by Sir Archibald Geikie on the "Founders of Geology."

Science reports the following gifts and bequests :—Mr. B. N. Duke has within the year given $183,000 to Trinity College; an anonymous donor has offered $25,000 for a biological laboratory for Vassar College, on condition that an equal sum be raised; according to the will of Mr. Jeremiah Halsey, the Norwich Free Academy receives nearly $100,000, and Trinity College, Hartford, $20,000; the Rev. H. Latham, of Cambridge, has given £2000 for the proposed Sedgwick Memorial Museum; Miss S. Dyckmann, $300 for a zoological scholarship in Columbia University for the present year; Dr. D. K. Pearson, $125,000 to Olivet College; Oberlin College has received $50,000 for a chemical laboratory, and two other anonymous gifts of equal amount; the late R. C. Billings of Boston left $100,000 to the Massachusetts Institute of Technology, and $50,000 for scholarships, besides $100,000 to the Boston Museum of Fine Arts.

We learn from *Science* that Mr. Charles H. Senff has given $5000 to the zoological department of Columbia University, which will be in part used for the publication of a memoir on *Polypterus*, to be undertaken conjointly by Messrs. Bashford Dean, Harrington, M'Gregor, Strong, Herrick, and Wheeler. Messrs. Harrington and Sumner hope to make a second expedition to the Nile in search of the fish. Prof. E. B. Wilson's recent efforts to obtain the eggs were disappointed.

A compromise has been effected in regard to the contested will of Dr. Robert Lamborn, and the Philadelphia Academy of Natural Sciences will receive over 300,000 dollars, or half of the testator's bequest.

In *Science* of June 30 there are details of the magnificent endowments of the Leland Stanford Jr. University, said to be the richest University in the world. Mr. Stanford left $2,500,000 in cash to the University, Mrs. Stanford deeded her own private fortune of about a million dollars, and has recently transferred the residue of the estate, which would probably bring in the market about $13,000,000.

At the second of the two annual conversazioni of the Royal Society on June 21, Prof. E. Ray Lankester exhibited collections of mosquitoes received at the Natural History Museum for study in reference to the connection between mosquitoes and malaria; Dr. P. Manson exhibited the malaria parasite; Messrs. Walter Gardiner and A. W. Hill showed intercellular bridges in plant tissues.

The Geologists' Association is the real centre of geological activity in London, in that it practically demonstrates the science in the field, and thus is an educational institution of real value. It is as active as ever in making excursions to places of geological interest in England, and has for the fourth time visited the Continent. Last Witsun, Dr. Barrois took a large party over the Brittany district, seeing to every detail of interest and comfort in the most careful way. The long excursion will be spent in Derbyshire from August 3 to 9 under the general guidance of Mr. Arnold-Bemrose, and promises great things for those especially interested in matters carboniferous.

At a meeting of the Royal Society of Edinburgh on July 17, Sir John Murray gave an interesting account of the progress which has been made in the hydrographic survey of Scottish lakes conducted by Mr. Pullar and himself. The biological contrast between the deep and shallow lakes was touched on, and its probable partial dependence on differences of temperature was hinted at. Dr. Hepburn exhibited a simple "osteometric board" for the more accurate and uniform measurement of bones.

At a meeting of the Royal Society of Edinburgh on July 3, Dr. Hepburn submitted an improved form of craniometer for the measurement of the transverse, vertical, and antero-posterior diameters of the cranium. Dr. Hepburn claimed that by means of this improved instrument measurements of the cranium would be more mathematically accurate, and that fully a dozen more measurements might be taken by means of it than had hitherto been

taken in connection with the skull. In Dr. Hepburn's instrument a graduated bar has been arranged to present zero at its centre, from which the figures proceed in duplicate in opposite directions. Opposite the centre of the graduated bar a straight pair of callipers has been introduced. Dr. Hepburn stated that, so far as he was aware, this was the only form of callipers to which the principle of a third limb had been applied. At the same meeting Professor Sir William Turner gave communications on the Craniology of the People of the Empire of India, the Hill Tribes of the North-East Frontier and the People of Burma, and on Decorated and Sculptured Skulls from New Guinea.

At the annual general meeting of the Marine Biological Association, held in the rooms of the Royal Society on June 28, the president, Prof. E. Ray Lankester, in the chair, it was noted that seventeen naturalists and eleven students had worked in the laboratory during the past year.

The Science Section of the Women's International Congress was held in the Westminster Town Hall on June 29, and Mrs. Ayrton occupied the chair. Astronomy was represented by Mlle. Klumpke from the Paris Observatory; geology by Miss Raisin, of Bedford College; chemistry by Miss Dorothy Marshall, of Girton; bacteriology by Mrs. Percy Frankland; and biology by Miss Ethel Sargant.

The forty-eighth meeting of the American Association for the Advancement of Science will be held at Columbus, Ohio, from the 21st to the 26th of August, under the presidency of Prof. Edward Orton. Ten societies in affiliation with the Association will meet at the same time.

The fourth international congress of Psychology will be held at Paris from the 20th to 25th August 1900, under the presidency of Prof. Th. Ribot. Prof. Ch. Richet will be vice-president, and Pierre Janet general secretary.

It is a cause for much gratification that the Government has conditionally promised £45,000 for the Antarctic expedition. This still leaves much to be raised, since the best authorities declare the minimum necessary to be £100,000. The Queensland Parliament is to be asked for £1000.

The Liverpool School of Tropical Diseases sends out their newly appointed lecturer, Major Ross, to the West African Coast to investigate malaria and other diseases.

The New Mexico Biological Station in charge of Mr. T. D. A. Cockerell is being conducted this summer at Las Vegas. Geological, anthropological, and botanical, as well as zoological, work is being carried on.

Col. W. S. Brackett, of Peoria, Ill., has organised an expedition of twelve mountaineers to explore the geological features of the almost unknown region between Buffalo Hump, in Idaho county, and the Nez Pierce Pass, in the Bitter Root range.

The United States Fish Commission is about to send out an expedition on the "Albatross," in charge of Prof. Agassiz, to explore portions of the Pacific Ocean. Some of the islands to be visited are the Marshall, Society, Friendly, Fiji, and Gilbert groups. It is expected that the trip will require eight months. The party will leave San Francisco in August.

There are already four polar expeditions under way, or almost ready to start, and to these must soon be added that of Capt. Bernier, a Frenchman. His course will be toward Franz Josef Land, for the part lying to the east of Cape Mary Harmsworth. After pushing on as far north as possible, he will disembark with all the provisions, dogs, reindeer, sledges, etc. He intends to pass the winter at Petermann's Land, and at the first opportune moment to make a dash for the pole.

Prof. W. A. Setchell, of the University of California, and some other botanists have gone on an expedition to study the flora of the Aleutian Islands.

Among those who have gone Arctic exploring are Professor W. Libbey, of Princeton, and Dr. R. Stein, of the U.S. Coast and Geodetic Survey.

Those interested in Antarctic exploration look forward with eagerness to the International Congress of Geographers in Berlin, when Sir John Murray, Sir Clements Markham, Dr. Nansen, Prof. von Drygalski, and others will meet and confer.

The Union Pacific Railway Company arranged in June a geological and palaeontological excursion to the fossil fields of Wyoming under the general direction of Prof. Knight, of the University of Wyoming.

It is noted in *Science* and elsewhere that Nansen has resolved to organise an Antarctic expedition for 1902, in which he will endeavour to supplement the work of the British and German expeditions.

On June 27 Prof. Virchow opened the Virchow Pathological Museum in Berlin, which houses his magnificent collection of specimens.

A pathological laboratory is to be erected at Oxford, the curators of the university chest having authorised an expenditure of £10,000, in addition to £5000 from an anonymous member of the university.

According to the *American Geologist*, the Minnesota Academy of Natural Sciences will send to the Greater American Exhibition at Omaha a collection illustrating the natural history of the Philippines.

The Dresser Collection of Birds has, we learn, been acquired by the Manchester Museum. Neither trouble nor expense were spared by the author of the "Birds of Europe" to make the collection as complete as possible, and more particularly to make it a working collection, and numerous specialists who have had the privilege of making use of it have united in expressing their opinion of its value in this particular direction. In addition to the European birds and the allied species from the Palaearctic region generally, it contains the materials used by Mr. Dresser in preparing his monographs on the bee-eaters and the rollers. As regards the extent of the collection, there are of bee-eaters about 30 species and 155 specimens, and of rollers 26 species with 112 specimens, whilst the Palaearctic collection contains from 850 to 900 species, or more according to the British Museum catalogue. When it is remembered that in almost every instance these forms are represented not merely by one skin, but by several showing the differences of plumage due to sex, age, and local variation, it will be readily believed that the collection includes about 10,000 specimens. There are several types and numerous rarities, among which may be mentioned two specimens of the rosy gull, whose nesting-place was discovered by Nansen in Franz Josef Land, and two Labrador falcons. The skins have all been carefully selected, and the collection has been accurately labelled, all particulars as to habitat and other details being recorded. Many specimens have been compared with rare types and noted as agreeing with them; others are the first or the only recorded specimens that have occurred within the western Palaearctic area. Enough has now been said (we quote the *Manchester Guardian*), to show that the acquisition of this valuable collection is indeed a piece of singular good fortune for the Manchester Museum, and therefore for all students of ornithology in the neighbourhood, and to call forth expressions of gratitude towards the generous benefactor who has rendered it possible for the museum to possess itself of such treasures.

Considerable changes have recently been made in the arrangement of the zoological collections at the Science and Art Museum, Dublin. These are in two rooms, the upper having a gallery round it. The upper room contains the general zoological collection, systematically disposed. The visitor is supposed to ascend to the gallery by a staircase marked No. 1; this lands him opposite the Protozoa. Thence he follows the gallery round from left to right, viewing on his way the various phyla of Invertebrata in ascending order.

Descending by another staircase, he finds himself close to the Tunicata, and so passes down the room between members of the whole Chordate series up to man. A noticeable feature of the arrangement is the position of the Echinoderma at the head of the Invertebrate series—that is to say, next to the lowest Chordata, with which they are supposed to be in a measure connected, owing to resemblances in the larval forms. The lower room is divided into three sections by large cases placed back to back. Section A contains collections illustrating the facts of geographical distribution ; Section B contains the Invertebrata, and C the Vertebrata, of Ireland, and in this series an attempt is made to display every species of the Irish fauna. Exigencies of the museum building have rendered it necessary to maintain the fossils as a separate collection ; this also is arranged systematically, on a similar plan. A guide, sold for ½d., instructs the casual visitor as to the route he should follow in order to obtain some idea of the classification of the animal kingdom.

The meeting of the Museums Association held at Brighton from the 3rd to the 6th of July, though not largely attended by either members from a distance or local well-wishers, was distinguished by the amount of serious work and discussion that was got through, and the absence of purely gaseous matter. The Mayor of Brighton made an excellent honorary president, and delegated the task of delivering an opening address to Mr. Henry Willett, who scattered over a wide field his suggestive and humorous remarks. Mr. G. H. Carpenter described the re-arrangement of the natural history collections in the Science and Art Museum, Dublin, and we give the gist of his paper in the above paragraph. Mr. H. Coats sent a note on a children's prize essay competition in the Perth Museum ; he seems to have met with success, but the idea of inciting children to this study by means of rewards, was opposed by some curators experienced in this matter. Mr. A. M. Rodger of Perth showed an insect box adapted for exhibition or for stowing away as a drawer. Mr. B. Lomax gave an interesting account of the perpetual exhibition of living plants in the Brighton Museum. Mr. J. V. Hodgson described the preparations for the new museum at Plymouth, and was asked by many of his fellow-curators whether nothing could be done to render the valuable Cottonian collection of art objects more accessible to students and to the public. Mr. B. H. Mullen again brought up the subject of a directory to the Museums of the United Kingdom, a work that would be of use to many besides the curators themselves. Mr. Harlan J. Smith sent some valuable suggestions as to the preservation of local archaeological evidences, as well as a description of the Museums of British Columbia, previously published in the *American Naturalist.* Another previously published paper was that on ink and paper for museum labels, by Dr. R. T. Jackson, which appeared in the *Proceedings* of the American Association for the Advancement of Science, and was now communicated by Mr. F. A. Bather, with the result of initiating a long discussion. A paper by Mr. Stewart Culin of Pennsylvania University gave a laudatory description of some museums in Dresden and Berlin. Museum preparations of marine animals by Dr. H. C. Sorby, ethnological photographs by Dr. H. O. Forbes, paper-covered tablets from the Horniman Museum, and a gorilla mounted by Brazenor Brothers for the Bristol Museum, were among the objects on view. During the week members visited the Brighton Museum under the guidance of Mr. Lomax, Mr. E. Crane, Mr. Thomas, and Prof. Boyd Dawkins ; and the Booth Bird Museum under the lead of Mr. A. F. Griffith ; the Aquarium, the apartments of the Pavilion, in which building the business and convivial meetings were held ; and finally, in charge of Mr. E. A. Pankhurst, they went as guests of the local committee to Lewes, where papers were read by Messrs. C. Dawson, J. Lewis, and G. de Paris.

We have received a prospectus of the exhibition of horticultural photographs which will be arranged in connection with the fourteenth "One and All" Flower Show at the Crystal Palace on the 14th and 19th August. In one class the

judges will for educational reasons depart from their custom and state the grounds on which the judgments are based. A statement of these will be affixed to the exhibits. The honorary secretary is Mr. Edward Owen Greening, 3 Agar Street, Strand.

We learn from the *Daily Chronicle* that when the coal boring was put down at Dover about six or eight years ago by Mr. F. Brady on the site of the old Channel Tunnel works, there were indications in the cores of the presence of iron ore in the strata between 500 and 600 feet from the surface. The indications have now proved correct.

In the course of sinking the No. 2 shaft, a bed of valuable oolitic iron ore has just been struck, at a depth of rather less than 600 feet. The seam proves to be no less than 12 feet thick, and probably extends over a very great area, the quantity being practically unlimited. The diameter of the shaft is 20 feet, and the quantity brought to the surface in passing through the 12 feet amounted to about 350 tons. Samples of the ore have been submitted to analysis, with highly satisfactory results, a washed sample of the ore yielding 45·8 per cent of iron. The analysis shows that the ore is free from sulphur and phosphorus, and it is stated to be of much richer quality than the Wealden ironstone worked in Kent and Sussex a century ago. Prof. Boyd Dawkins, in a paper read before the British Association in 1894, described a sample obtained from the original boring. From this it appears that this bed of iron ore is identical with that described by Blake and Hudleston at Abbotsbury in Dorset, where it occurs between the Kimmeridge clay above and the Coralline rocks below. It is also physically identical with the valuable iron ore worked for many years in Westbury, Wiltshire. The ironstone presents very singular physical characteristics. It is composed of dark brown, shining grains of hydrated oxide of iron, like millet seed, embedded in a crystalline base partly of calcium carbonate and partly of iron carbonate.

The last year has been, we learn from the *Scientific American*, the most successful in the history of the U.S. Fish Commission. Millions of shad, trout, cod, and other fry have been distributed. It is said that the cost of shad has been decreased to the consumer by more than 30 per cent.

The *British Medical Journal* publishes an inaugural lecture, delivered by Major Ronald Ross at the Liverpool School of Tropical Medicine, on the possibility of eradicating malaria from certain localities by killing off the mosquitoes (*Anopheles*) from the puddles.

We learn from *Nature* that the *Academy* invited its readers to compose an inscription of not more than forty words, suitable to be engraved upon the statue of Charles Darwin, recently unveiled at Oxford. The following, by Mr. Edwin Cardross, was considered best:—"Charles Darwin, the great naturalist, memorable for his demonstration of the law of evolution in organic life, achieved by scientific imagination, untiring observation, comparison, and research; also for a blameless life, characterised by the modesty, 'the angelic patience, of genius.'"

The *Scientific American* reports that the North Dakota Senate has passed a bill requiring all applicants for marriage licences to be previously examined by a board of physicians as to their mental and physical fitness. The certificates must show that they are free from hereditary diseases, with special reference to insanity and tuberculosis. "Legislation of this kind is interesting, but that is about all that can be said for it, for there is nothing to hinder the contracting parties from going over the border into adjoining States to have the ceremony performed."

Dr. Otto Thilo, Riga, Russia, makes an appeal for information regarding the fish *Thalassophryne*, which he wishes to investigate in connection with his work on poisonous organs.

Natural Science

A Monthly Review of Scientific Progress

September 1899

NOTES AND COMMENTS.

Integration in Science.

Under this title Sir Michael Foster delivered a stirring address to the Yorkshire Naturalists' Union last December, and the address is now reprinted in full in *The Naturalist* for July. Its object is to consider how Naturalists' Societies may be used to check the tendency of biological science to disintegrate into separate and distinct sciences, and to show how far that disintegration has already proceeded, and how great the need for integration. Sir Michael compares the Temple of Science to that earlier erection which men are said to have built on the plain of Shinar. Both buildings seem to have the same consequences, in that, as they rise, the builders cease to understand one another's tongues. What then shall the modern workmen do to prevent the fate of their prototypes becoming their fate also? Has not the confusion of tongues already proceeded so far that the workmen are scattered and the building delayed? As Sir Michael points out, not only have physicist and chemist learnt to speak a language unintelligible to botanist and zoologist, but worse still, the erstwhile zoologists are split into anatomists, physiologists, and systematists, each of whom uses a tongue foreign to his brother. The extension of the examination system has aggravated the evil, until to many a "zoologist" the animal form is seen only through "the long vista of a lengthy ribbon of gorgeously stained microtome-cut sections of exquisite thinness." That much of this is the necessary consequence of the division of labour and the progress of knowledge cannot be denied, nor can we forget that the "outcome of the deepest, most far-reaching biologic inquiry has been the rehabilitation of the naturalist of old," yet the reality and extent of the evil can hardly be overestimated. Sir Michael is of opinion that there is little hope of remedying it by an appeal to the schools, but he thinks that it is the special function of Naturalists' Societies to assist in the process of integration, and to teach the academic neophytes something of the meaning of the word naturalist. The moral is so excellent that it seems worthy of the attention of societies other than that to which it was addressed.

Women and the Learned Societies.

At the recent International Congress of Women in London, Mrs. Farquharson of Haughton, in the course of a paper on the work of women in biological science, drew attention to the fact that at least three of the large scientific societies still refuse to admit women to their full fellowship, however fully qualified they may be. These three societies are the Royal, the Linnean, and the Royal Microscopical. Of these the Royal Microscopical admits women to its membership, but refuses to permit them to attend its meetings, while the two other societies entirely refuse membership on any terms. Mrs. Farquharson dwelt upon the hardship thus entailed upon women in special cases.

British Botany.

That much still remains to be done in the field of British Botany—at any rate among the lower plants—is evident from papers which have recently appeared in the *Journal of Botany.* In the May number of the *Journal,* Mr. Gepp notes the occurrence of no less than four aquatic fungi, hitherto unrecorded from Great Britain, which were found growing on a broom-handle floating in a reservoir near Shrewsbury. These fungi belong to the genera *Achlya* and *Apodachlya,* of the family Saprolegniaceae; and there is little doubt that a careful study of the native members of this group, on the lines suggested by the writer, would result in other interesting finds.

The July number of the same Journal contains a description and figure of a fresh-water Alga, which forms not only an addition to the British flora, but a variety new to science. It is a filamentous green Alga allied to the common *Cladophora,* and forming, like the latter, masses of tangled green threads, but of finer consistency and a brighter green. It belongs to the genus *Pithophora,* the history of which is of some interest. The genus was founded by the Scandinavian botanist Wittrock, on a plant which appeared some years ago in the water-lily tank at Kew, and had presumably been introduced from the Amazons along with the lilies. Wittrock subsequently described several other species from various parts of the world. The original one has long since disappeared from Kew, and has not been found elsewhere; but another, the subject of the communication, has recently appeared in the Reddish Canal, near Manchester. This canal is a classical locality, having supplied a new *Chara,* and also become the home of an aquatic monocotyledonous flowering plant, *Najas graminea.* The latter is widely spread in the tropics of the Old World, and has also long been known from Northern Italy, where it is generally supposed to have been brought

from Egypt with rice. It is suggested that its presence near Manchester is due to an introduction of the seeds along with Egyptian cotton, and this view is supported by the fact that the Manchester plant resembles Egyptian specimens in a certain anatomical detail of the leaf-structure. The new Alga was growing attached to the stem and leaves of the *Najas*, and may have been similarly introduced; but, so far, the genus *Pithophora* has not been recorded from North Africa.

Polemics and a Parasite.

The *Zoologischer Anzeiger* for July 3 contains an article by Professor W. M. Wheeler entitled "J. Beard on the Sexual Phases of Myzostoma" (pp. 281-288), which is a fine example of polemical discussion. We all like a fair fight, even if we won't admit it; and perhaps these zoological tilts are like the combats of male spiders in this, that neither party is wounded. Wheeler criticised Beard, and Beard criticised Wheeler, and the bystanders were edified; and we cannot but say that the edification continues as Wheeler returns to the charge. Our only doubt is as to the wisdom of using words that have a moral connotation, words like "garble" and "misrepresent," which we see in the paper before us. A more philosophic note is struck when Mr. Wheeler expresses the hope that "continued controversy may induce some student (we omit the adjective conscientious) who has an opportunity of working at the Naples Station or at the French or Japanese sea-side laboratories, to undertake a renewed study of the reproductive organs of the various species of *Myzostoma*."

But what is the dispute about? Beard holds that *M. glabrum* is dimorphic, the species being represented by hermaphrodite individuals and by dwarf complemental males. The latter are dorsicolous, that is, they are attached to the dorsal surface of the large hermaphrodite individuals which in turn adhere to the peristome of *Antedon rosacea*.

From a comparative study of several species representing the morphological extremes of the genus *Myzostoma*, Wheeler concluded that *M. glabrum* is monomorphic, each individual being from the first hermaphrodite, *i.e.* possessing both ovaries and testes, and being like other members of the genus (notably *M. cirriferum* and *M. alatum*) protandrous, then hermaphrodite, and ultimately more or less hysterogynic. "In other words, the functional male phase (Beard's complemental male) passes into the functional hermaphrodite phase as soon as the first ova mature, and the functional female phase begins with the atrophy or disappearance of the testes. The cysticolous and endoparasitic species of the genus tend towards a condition in which the functional male and female phases overlap but little, thus exhibiting only a brief functional hermaphrodite phase (*M. cremita*), or these

phases no longer overlap and thus present two well-marked periods of sexual maturity, one male and the other female (*M. pulvinar*)." This Mr. Wheeler regards as a simpler and more satisfactory "explanation" (or rather description) of the sexual peculiarities of *Myzostoma* than has been offered by Beard or any other author. He proceeds to criticise Beard's critique, and ends up by expressing the hope that "every fair-minded zoologist will be convinced that the complemental male of *M. glabrum* is one of those tenuous and fanciful creations for which one could have wished that euthanasia, that silent death so becoming to pet speculation when they have ceased to afford either amusement to their originator or edification to their readers." The temperature of Chicago is high!

Life High and Low.

A SUMPTUOUS French translation has been published of an essay by Prof. A. L. Herrera and Dr. D. Vergara Lope, on life on the high plateaux[1]—an essay which gained honourable mention and a silver medal in the competition for the Hodgkins prize of the Smithsonian Institute in 1895. After a general discussion of plateaux, the authors consider the vertical distribution of plants and the adaptations exhibited by those living at high altitudes. They then pass to the vertebrate animals composing the plateaux-fauna, and show that here also special adaptations may be detected, especially perhaps in the function of respiration. Man's life on the heights is then considered, and many facts are cited and suggestions offered as to the therapeutic value of a residence on the plateaux. The work is laboriously erudite and carefully planned, and will be a welcome addition to the consulting library of biologist and physician alike. Against the old theory that life at high altitudes is too difficult both for man and beast to be healthful, and that it brings about degeneration of body and mind, the authors argue most strenuously. Their central thesis is that plants, animals, and man may become acclimatised to high altitudes, and live a life of full vigour "obeying the eternally true law: *Semper ascendens.*"

It is a far cry from the Mexican plateaux to thirty fathoms below the Eddystone lighthouse, but the naturalists' problem is the same: how are the organisms adapted to the peculiarities of their environment? Mr. E. J. Allen, director of the Plymouth Laboratory, has been investigating for some years the distribution of the fauna on the sea-bottom along the thirty-fathom line from the Eddystone Grounds to Start Point, with the particular object of ascertaining and, where possible, explaining the changes which take place in the animal

[1] "La vie sur les hauts plateaux," pp. 790, 18 tables, numerous plates. Mexico, 1899.

population when the nature of the bottom deposit changes. It has been a laborious piece of work, executed with patient carefulness, and the results though not startling are certainly valuable.[1]

Since the principal object of the investigation was to study the relation of the fauna to the bottom-deposit, the area selected for examination was so chosen that the general physical conditions were uniform apart from the nature of the deposits, and the amount of disturbance of the bottom water by the action of waves was relatively small. The chief results to be gained by carefully scanning the numerous tables—the drawing up of which must have meant a large amount of work—relate to the suitability of certain kinds of ground for certain kinds of animals, but apart from this the memoir is also interesting because of the numerous notes on the habits of the animals and for its analysis of the environmental conditions.

The physical conditions, the variations of which influence the life of bottom-living species, are capable of definite statement, and for the most part of accurate measurement. They are—

1. The constitution of the sea-water.
2. The nature of the bottom-deposit.
3. The movements of the water, due to
    (*a*) wave action,
    (*b*) currents,
    (*c*) tides.
4. The temperature of the sea-water.
5. The pressure, varying with the depth of water.
6. The amount of light which penetrates to the bottom.

The external biological conditions influencing the distribution of any bottom-loving organism, due to the existence at the same time of other living organisms, are often of a complicated nature.

1. One organism may exert an advantageous influence upon another.
    (*a*) By serving as its food-supply;
    (*b*) By serving as a fixed base to which it may attach itself;
    (*c*) By serving as a movable base, and thus extending the area over which a fixed organism can collect its food-supply;
    (*d*) By bringing supplies of food to the other organism as well as to itself, either by setting up a current, or in some other way;
    (*e*) By affording the other organism means of protection or concealment from its enemies.
2. One organism may exert a disadvantageous influence upon another.
    (*a*) By preying upon it;

[1] "On the Fauna and Bottom-deposits near the Thirty-fathom Line from the Eddystone Grounds to Start Point," *Journ. Marine Biol. Ass.* v. June 1899, pp. 365-542, 15 charts and 7 tables.

(*b*) By fixing upon it in such a way as to destroy it;

(*c*) As a competitor for a limited food-supply, or for a limited amount of fixing space.

3. The biological conditions by which the organisms on any particular patch of ground are influenced depend not only upon the organisms living on that ground itself, but also upon the nature and abundance of the organisms living upon neighbouring grounds.

We have quoted the above analysis because it seems to us admirable, and indicative of the careful manner in which Mr. Allen has dealt with his problem. And although the research has a less obvious practical outcome than that on plateau-life, with which we have coupled it, this justification is not awanting, for it helps towards an understanding of the local distribution of food-fishes.

As Regards Protoplasm.

THOSE acquainted with Prof. E. B. Wilson's work entitled "The Cell in Development and Inheritance" will remember that he is no optimist, and will not be surprised to find him saying in a more recent deliverance (*Science*, x. 1899, pp. 33-45, 4 figs.):—"If we except certain highly specialised structures, the hope of finding in visible protoplasmic structure any approach to an understanding of its physiological activity is growing more, instead of less, remote, and is giving way to a conviction that the way of progress lies rather in an appeal to the ultra-microscopical organisation and to the chemical processes through which this is expressed." He starts in his lecture with a familiar object—the egg of the sea-urchin—and defines the problems suggested by it: (1) What is the actual structure that gives the appearance of a meshwork? (2) How faithfully does the preserved structure, as seen in sections, reproduce that existing in life? (3) What is the relation of the astral systems to it? (4) What is the finer structure and origin of the meshwork? (5) Can this structure be taken as typical of all protoplasm; and if not, what is its relation to other forms of protoplasmic structure? And incidentally, still another interesting question arises: Is it possible to identify any one of the three visible components—granules, continuous substance, ground-substance—as the living substance or protoplasm proper, as distinguished from a lifeless metaplasm, and, if so, what are its structural relations?

To propose dogmatic answers to these questions would be at present absurd, and Professor Wilson is of no such mood. He has, however, specialised in cytological work, and his conclusions are therefore of value to less intimately initiated workers.

As to the nature of the meshwork he concludes that in the resting

cell it is in reality an alveolar structure — an emulsion — such as Bütschli has described. The living stuff of an Echinoderm ovum is in the form of a fine emulsion consisting of a continuous substance in which are suspended drops of two orders of magnitude and of different chemical nature, the larger drops determining the primary alveolar structure as described by Bütschli, the smaller drops determining the secondary or finer alveolar structure as described by Reinke. As to the astral rays in the sea-urchin egg and elsewhere, they involve a radial arrangement of the alveoli, but they involve more, namely, definite fibrillae which grow by progressive differentiation out of the general cytoplasmic meshwork.

The phrasing of the last sentence suggests a more general conclusion — "that alveolar, granular, fibrillar, and reticular structures are all of secondary origin and importance, and that the ultimate background of protoplasmic activity is the sensibly homogeneous matrix or continuous substance in which those structures appear." Not that the author puts his finger upon this, so to speak, and says this is *the* living matter, for "in its fullest meaning the word living implies the existence of a group of co-operating factors more complex than those manifested by any one substance or structural element in the cell, nevertheless, we are perhaps justified in maintaining that the continuous substance is the most constant and active element, and that which forms the fundamental basis of the system, transforming itself into granules, drops, fibrillae or networks in accordance with varying physiological needs." Thus we are led to the conclusion that the physical basis of life is in the invisible organisation of a substance which seems to the eye homogeneous. Beyond this, as far as morphological aspects are concerned, all is hypothesis, and the form of hypothesis which Professor Wilson favours is "that the homogeneous or continuous substance may be composed of ultra-microscopical bodies, by the growth and differentiation of which the visible elements arise, and which differ among themselves chemically and otherwise, as is the case with the larger masses to which they give rise."

The Darmstadt Museum.

Although the new building of the Grossherzogliche Museum at Darmstadt is unfinished and untenanted, the plan of the zoological portion has been carefully worked out by Dr. G. von Koch, the director, and some idea of its main features can be gained from his programme and from the newer cases in the old museum.

In the "Schausammlung" or show collection intended for general instruction, there is of course a systematic series, but prominence is given to cases showing things more or less as they are in nature or

grouped to illustrate some particular fact or adaptation. Thus we see a beech wood in winter with its withered leaves, squirrels, and woodpeckers; the bank of a stream with its wagtails, kingfishers, and other tenants; a tree with distinctive nests at the various levels, and so on.

Other cases—more difficult to work out naturally—are beginning to illustrate geographical distribution, so that he who runs—and such is too often the museum pace—may almost read. The posing of many of the birds, such as the albatross, in flying attitude; the juxtaposition of the stuffed creature and its skeleton (as in the case of *Ateles geoffroyi*); the arrangement of lenses over selected corals; the models showing musculature in natural size, *e.g.* of the elephant's skull and fore-limb, and other features, struck us as we walked through, and lead us to look with expectation to the opening of the new museum. Dr. Koch evidently believes in keeping the detailed collection for workers in a form which will be convenient to the student and will save the laity from embarrassment, and in making each exhibit of the so-called show collection really teach something.

An Annelid from the Devonian.

THE lamentable condition of fossils found in the Devonian rocks of the south coast of Cornwall makes a communication by Mr. Upfield Green to the Royal Geological Society of Cornwall of more than ordinary interest. This consists of a brief record with figures of the impression of an annelid to which he has given the name of *Nereitopsis cornubicus.* The specimens come from the slates of Polruan, Polyne, and two unknown localities, and are four in number. They are identical in structure, and are certainly impressions of different individuals of the same species. As Mr. Green has not ventured to describe them, it may be well to offer a few remarks on the original specimens, which are faithfully represented by the figures of life size. From the central rod, now represented by a hollow, and which shows traces of segmentation, spring pairs of impressions of parallel striae, the distal end of each of which terminates in a > shaped point. Each pair of impressions increases in size from the tail towards the head (not seen in any of the specimens). The tail appears to have a swollen and tuberculated aspect, but is obscure. Such in few words is a description of these curious fossils, which have been illustrated and published in the hope that better material may be forthcoming now that attention has been drawn to them. The originals are in the Museum of the Royal Geological Society of Cornwall at Penzance.

Cultivation of the Vine for Wine in Essex.

A QUESTION relative to the above heading was asked in the *Essex County Chronicle* for Dec. 9, 1898, and has produced a paper on the subject in the *Essex Naturalist* (Jan.-March 1899) from the pen of Mr. Miller Christy. This paper, which is of considerable interest, deals with the matter historically, and collects together a great deal of valuable information. For instance, no fewer than eight records of vineyards in Essex occur in Domesday Book, and other records occur for 1130, 1252, 1303, 1380, 1540, 1667, etc. Wine was produced, according to these records, in 1086, 1130, and 1667, the produce of the latter year being mentioned by Pepys as grown at Walthamstow. Reference is made to the place names, and to hop-growing, and to the fact that the vine is largely grown at the present day for the sale of the grapes themselves, rather than for the wine the grapes might yield.

Did Palaeolithic Man Inhabit Scotland?

IN a brochure by the Rev. Frederick Smith of Cromlix, entitled "Some Investigations into Palaeolithic Remains in Scotland" (a reprint from the *Proceedings of the Philosophical Society of Glasgow*, read 30th November 1898), the author claims to have discovered palaeolithic implements in many localities throughout Scotland, including the valleys of the Forth, Tay, Earn, Allan, Dee, and Don (Aberdeen), as well as the Clyde estuary.

That such implements have not been hitherto recognised in Scotland is, according to Mr. Smith, due to the fact that "the searchers were looking for the wrong thing. The accepted forms being of flint, flint specimens were sought in Scotland; or, on the supposition that other materials than flint might have been used, specimens of equally fine form and elaboration were expected. But no flint exists in Scotland; hence flint specimens could not have been anticipated." No objection can be taken to the logic of the above statement, but it is equally certain that if palaeolithic man did not inhabit Scotland, as has hitherto been assumed, the products of his hands need not be looked for. With regard to Mr. Smith's reported discoveries, the main question which has to be determined is, whether the objects are, or are not, of human workmanship. Should this be decided in the affirmative the next step would be to ascertain if they were actually found in circumstances which would lead us to regard them as the handiwork of Palaeolithic Man? On both these points the author is very confident of a favourable verdict. He tabulates his results as follows:—

(1) "Angular—*i.e.* unrolled—stones, in shape similar to the flints

of the Somme, but wanting the characteristic flaking, were found in the soils of the higher areas of the lower Tay valley, but were entirely absent from those of the 50-feet and lower terraces."

(2) "Similar stones found in Kaims and the most ancient river deposits, but more or less rolled or water-worn."

(3) "These stones entirely absent, under ordinary circumstances, in recent river deposits; if present, so completely water-worn as to be practically unrecognisable."

There exists, no doubt, a borderland, in which it would be difficult to distinguish natural productions from the ruder works of man; but so long as this indefiniteness characterises Mr. Smith's specimens, no archaeologist would be justified in concluding from them as to the presence or absence of man in the district. Until this problem is settled we need not inquire into the merits of the subsidiary one. For the clear, methodical, and terse manner in which Mr. Smith has laid the facts before the public he deserves a word of encouragement, but we cannot say that he has proved his case.

Insects and Tobacco.

THE Year-Book of the U.S. Department of Agriculture for 1898 contains an interesting paper by Dr. L. O. Howard on insects injurious to the tobacco plant. It is remarkable that this plant, though native in North America, is less subject to insect ravages than are cereals and other imported crops. The most destructive of the enemies mentioned here is a small "flea-beetle," *Epitrix parvula*, which eats holes in the leaves, and renders them liable to further damage through the entrance of fungus-spores. The caterpillars of two large hawk-moths and of several noctuids, including species so familiar to British entomologists as *Agrotis saucia* and *Heliothis armigera*, are also noticed. Even when prepared for consumption in another way by vertebrate admirers, tobacco is still sought after by hungry arthropods; the "cigarette beetle," *Lasioderma serricorne*, bores into all kinds of stored tobacco. "An entomological acquaintance," writes Dr. Howard, "insists that he buys infested 'short cut' by preference, both because he can get it cheaper, and because the bodies of the insects impart a distinct and not disagreeable flavour to the tobacco. He admits, however, that it is a cultivated taste."

Ichthyosaurus at Home.

One of the shortest cuts to a realisation of *Ichthyosaurus* is a journey to the Museum in Stuttgart. It may be that the Saurian's rehabilitation is still caviare to the general, but there are many accessory attractions by the way. The Stuttgart Museum—the Naturalien-Cabinet as they call it—is indeed a treasure-house for students of palaeontology, whether they are interested in tertiary mammals or the teeth of *Microlestes*, crustaceans or Steinheim molluscs, Labyrinthodonts or Saurians, and it is said that the thicket of mammoth tusks from Cannstadt has proved so impressive that it is mentioned in Baedecker, which surely means an Ultima Thule of fame.

The museum as a whole is painfully suggestive of what museologists call "the fat boy," except in this respect that it seems in no wise somnolent. But it puzzles the inquisitive visitor to imagine where a single additional specimen could possibly be stored. The most ingeniously crowded cases of "Vermes," for instance, are positively heartrending, and one feels that a few more exchanges would leave only the labels visible on the ascending staircase of bottles.

Among the striking features may be noted the extraordinarily rich series of Pheasants and Birds of Paradise; the fine representation of the Württemberg fauna, including that strange phenomenon—Rattenkönig—of many rats entangled by their tails, and with a wealth of duplicates, *e.g.* of *Pelias verus*, which must surely embarrass anyone but a student of variations; a skilfully displayed set of insects injurious to herbs and trees; besides various fascinating rarities like the Great Auk.

Yet *the* feature of the collection is doubtless the series of Saurians (in the wide sense) on which Dr. Fraas—one of the custodians of the museum—has worked with so much success. It was among these that we recently spent two happy forenoons, and it was the wealth of species and individuals of Ichthyosaurus—from one measuring twelve metres in length to a little foetus within its mother—which suggested the title of our note, written not for the learned palaeontologist at home, but for the amateur naturalist abroad, in the hope that among the thousands of English visitors who pass annually through the charms of Stuttgart, this may possibly arrest some to enjoy the glimpse into an ancient world which the palaeontological museum affords. There are of course many richer collections, but it will be hard to find one equally rich of which it can be said that all the treasures are local. Perhaps even the Stuttgarters themselves are but dimly aware that the Naturalien-Cabinet is a much more marvellous treasure-house than even the wonderful Moorish Palace of which they are justly proud. Similarly, there are but few elect Dundonians who have any notion of the wealth of Prof. D'Arcy Thompson's collection

in University College. Our point, however, was that to realise Ichthyosaurus, to see it disporting itself with its flukes, to verify its dorsal fins, to inquire into the contents of its stomach, to peer even into its oviduct, one must go to Stuttgart and sit at the feet of Fraas.

A Note on Zoos.

AGAIN and again it has been remarked that zoological gardens flourish on the continent in towns whose population is less than that of British centres in which the institution of a "Zoo" would be regarded as foredoomed to failure. The reasons for this are doubtless manifold:—the treacherous British climate is largely to blame; we are given to take our pleasure sadly; there is the little item of delectable uninjurious beer with which British brewers still leave us unprovided, and so on.

The pros and cons have been often discussed, and we have had some opportunity of considering them. Our verdict is that a "Zoo" would flourish and pay in Edinburgh, for instance (where the project has been recently discussed with more or less vague enthusiasm), just as well as in Stuttgart, if only a company would select a scientific person with brains to run it.

After visiting the garden in Frankfurt, which is in some ways almost luxurious in its wealth of exhibits, we were glad for our country's sake to see the little Nil-Garten at Stuttgart. For Edinburgh all at once to start a zoological garden on the scale of the Frankfurt one is as unlikely as that there should be an independent Edinburgh Antarctic Expedition; but that a company of enthusiastic Edinburgh naturalists and business men should not be able to run as good a garden as there is in Stuttgart is absurd.

So far as we could gather, it seems to be "run" by one man, and there were few irrelevant attractions. Yet the garden was an interesting one, with its *Echidna*, a very fine *Myrmecophaga jubata*, a sloth, an orang, a chimpanzee, the usual galaxy of monkeys, a fair sample of carnivores and ungulates, a lot of quite happy birds, a great somnolent giant salamander and silurus, and so on.

There was not perhaps anything new to the expert naturalist, but there was enough for even his observation for an hour or two. The collection seems to have started with monkeys, but it has broadened out, and it is at once a credit to the town and an example to others who might go farther for suggestion and fare worse! One thing, however, a visitor to the Stuttgart garden must feel, that without a good water-supply a thoroughly successful and beautiful Zoo is impossible.

ORIGINAL COMMUNICATIONS.

The Original Rock of the South African Diamond.

By Professor T. G. Bonney, D.Sc., LL.D., V.P.R.S.

In 1867 the first diamond was discovered in South Africa, one having been found in some gravel from the Orange River. Three years afterwards it was obtained in a peculiar deposit of a yellowish colour, like a rotten, rather saponaceous shale, about 15 miles away from the stream and near the present site of Kimberley. There was a rush to the spot, and excavations were soon opened. For some time the mining places were only four in number, and near Kimberley; a fifth was afterwards added, but all of them lie within a circle of about $3\frac{1}{2}$ miles in diameter. Since then similar deposits have been found elsewhere, and the Newlands Mines, in West Griqualand, to which I shall more especially refer, are about 42 miles to the N.W. of Kimberley. The diamantiferous "yellow ground," as the miners called it, was found, as it was worked downwards, to change gradually into a rather more coherent rock, of a dull dark green-blue colour, named "blue ground"; this became more solid as the workmen followed it downwards, till at a depth of 1200 to 1400 feet it is nearly as consistent as a limestone.[1] In this matrix the diamond occurs,[2] together with a number of other minerals, such as garnets (chiefly pyrope), olivine, pyroxenes (including enstatite, chrome-diopside, and smaragdite), a brownish mica passing locally into a chlorite, ilmenite, and magnetite, with small fragments of zircon and kyanite.[3] The ferro-magnesian minerals are more or less serpentinised, and the pyropes are often surrounded by a kelyphite rim, much of it consisting of brown mica. The diamonds, it may be added, are often found, by their

[1] I believe that 1800 feet has been reached in the De Beers Mines, but I have not heard whether the hardness of the rock has materially increased ; probably it has not.

[2] According to the De Beers Consolidated Mines Report, 1889-90, the average yield in that mine is from $1\frac{1}{5}$ to $1\frac{1}{3}$ carats per load (about 1600 pounds) ; the Kimberley is much the same. In Bulfontein and Du Toit's Pan it varies from $\frac{1}{6}$ to $\frac{1}{3}$ of a carat per load.

[3] See Lewis, "Genesis and History of the Diamond," for a very full history and account of the minerals, large and small.

anomalous optical character, to be in a condition of strain, and they are sometimes only fragments of crystals.

The matrix, in which the above-named minerals are rather irregularly scattered, consists of serpentine, somewhat fragmentary in aspect, mixed with about 16 per cent of a carbonate—calcite or dolomite, granules of iron oxide and perovskite; sometimes tiny flakes of brown mica—apparently of secondary origin—are generally disseminated. To some investigators the rock seems to be porphyritic, to others brecciated, several of the minerals looking rather rounded. Angular rock fragments—shales, grits, diabases, and the like (the first of these sometimes apparently a little altered)—are also present, though in variable quantity. The country rock is a shale, often dark, interbedded with hard grits, and associated with flows or sills, and with dykes of igneous rocks, mostly basalt or diabase. Dykes also occasionally cut the diamantiferous rock. The latter occurs in pipes which bear a general resemblance to volcanic necks. These vary in size, the largest, named Du Toit's Pan, being about 45 acres in area.

This very brief sketch of the circumstances under which the South African diamonds have been hitherto found may suffice for our present purposes, since so much has now been written on the subject.[1] The facts which have been briefly summarised have received very diverse interpretations, though all admit that the rock has been considerably affected by secondary mineral changes, which have been brought about, in all probability, by the action of heated water. Some writers, however, maintain that the rock is a breccia, and that the diamond, like the garnets, pyroxenes, olivines, etc., was formed elsewhere, the parent rock or rocks having been shattered by some form of explosion. Others, while taking the same view as to the character of the blue ground, believe that the diamond was formed *in situ*, probably by the action of highly heated water (under considerable pressure) on the carbonaceous material of the country rock (Karoo shale[2]). Others, again, agree with the late Professor Carvill Lewis in regarding the "blue ground" as a serpentinised and otherwise altered peridotite of somewhat peculiar form. For this he proposed the name Kimberlite, thus defining it "a porphyritic volcanic peridotite of basaltic structure, or, according to Rosenbusch's nomenclature, the palaeovolcanic 'Erguss form' of a biotite-bronzite-dunite, being an olivine-bronzite-picrite-porphyrite, rich in biotite . . . it is a rock *sui generis*, dissimilar to

[1] I think it needless to attempt a bibliography. The earlier more important papers, with some which cannot be so designated, will be found in Carvill Lewis's "The Genesis and Matrix of the Diamond," 1897. Some of later date are mentioned in my paper on "The Parent Rock of the Diamond in South Africa," read to the Royal Society on 1st June of this year. The classic paper of Professor Maskelyne and Dr. W. Flight (*Quart. Journ. Geol. Soc.*, xxx. 1874, p. 406) contains the first thorough investigation of the associated minerals, and much information will be found in De Launay, "Les Diamants du Cap," Paris, 1897, and in Max Bauer, "Edelsteinkunde," Leipzig, 1896, both of them most valuable works of reference.

[2] This is referred to the Triassic period.

any other known species. Three varieties of Kimberlite may be distinguished: (1) Kimberlite proper, a typical porphyritic lava; Kimberlite breccia, the same lava broken and crushed by volcanic movements and crowded with included fragments of shale; (3) Kimberlite tuff, being the fragmental and tufaceous portion of the same volcanic rock. These varieties pass by insensible gradations one into another, so that no sharp line can be drawn between them, and all occur together in the same neck or crater."[1] He held that the diamond was produced *in situ*, the basic magma of the peridotite offering so little facility for the oxidation of the carbon.

In this diversity of opinion two points had to be settled before the genesis of the diamond could be determined: (*a*) whether that mineral was authigenous—crystallised on the spot—in the so-called Kimberlite; and (*b*) what was the true nature of that rock. If it were a serpentine, there was then a high probability (though not certainty) that the diamond was authigenous and the date of its birth later than the Triassic period; if, however, the rock were a breccia (produced by some form of volcanic explosion), it was then more probable that the diamond, like many of the other minerals, had been obtained from the shattering of some more ancient crystalline rock.

My connection with this interesting and amicable controversy began in 1891,[2] when, at the request of Professor Rupert Jones, I examined with Miss C. A. Raisin some minerals and small rock fragments which he had received from South Africa. Of the former specimens nothing more need be said since they were those usual in "washings"; but the latter were clearly pieces of a coarse eclogite, consisting mainly of a red garnet and a green augite (that now identified as chrome-diopside); both being minerals found in the Kimberlite. This investigation caused me to pay closer attention to the question, and the circumstances mentioned in the Preface to the "Genesis and Matrix of the Diamond," by my lamented friend Professor Carvill Lewis, led to my undertaking (with the kind aid of Professor Rosenbusch) to see his manuscripts on this subject through the press. But before these reached me I had the opportunity of examining two remarkably well-preserved blocks of the breccia, brought from Kimberley by Sir J. B. Stone, M.P. He kindly presented one of these to me, and a description of it and some other specimens is published in the *Geological Magazine*.[3] I came to the conclusion, as there expressed, that the

[1] "Genesis and Matrix of the Diamond," p. 50. I may add that neither in Professor Lewis's microscopic slices which I studied, nor in the rather numerous collection which I possess, some of them unusually well preserved, have I been able to recognise these three varieties. I have been for some years convinced that the rock was a breccia, and my latest studies (*Geol. Mag.*, 1897, p. 448) proved to me that certain fragments which I had thought might possibly represent a compact peridotite after serpentinisation, must have had quite another origin.

[2] *Geol. Mag.*, 1891, p. 412.

[3] By myself and Miss Raisin, with a prefatory note by Sir J. B. Stone, *Geol. Mag.*, 1895, p. 496.

rock was a true breccia. That opinion was not altered by the study of Professor Lewis's manuscripts, but I thought it possible that his Kimberlite might be represented in certain very compact fragments of serpentinous aspect, the nature of which I had been unable to determine, owing to the want of definite characters and to my own ignorance of what a serpentine formed from a glassy or very compact peridotite would be like. Apart from this possibility, my views on the main question differed from those put forward by my friend. It was, however, my obvious duty to keep the difference of opinion as far as possible in the background, and to endeavour to act as a simple channel for the publication of the views of one who was no longer able to speak for himself. Not long after the book had been published, Sir W. Crookes allowed me to examine a piece of breccia which had been obtained at a depth of 1320 feet, and was in even better preservation than any which I had hitherto seen. About the same time Sir J. B. Stone forwarded to me another set of specimens which he had received from Kimberley. Among these were two or three blocks, in almost as good a condition as that just named, and from an even greater depth, viz. 1400 feet. After study of these [1] I was more than ever convinced that the Kimberlite was a true breccia, formed by the explosive destruction of some coarsely crystalline rocks, such as eclogites and peridotites (including representatives of the sedimentary rocks of the region). I was also able to ascertain the true nature of those fragments which hitherto I had thought might possibly be serpentine of an exceptional character; they proved to be in reality nearer to argillites, but to have undergone certain alterations, in all probability partly from contact action, and partly from water, perhaps at a rather high temperature, and no doubt at a later time. Thus I arrived at the conclusion, that the so-called Kimberlite was not an altered peridotite, but a breccia, in which the diamond, like the olivine, pyroxenes, garnet, etc., was not authigenous, but a derivative from some older rock. This I thought very probably was a peridotite, for an *a priori* argument, as we may call it, which Professor Lewis had used seemed valid, even though he might have misunderstood the nature of the Kimberlite, and his idea that a very basic rock would be the birthplace of diamonds was confirmed by their occurrence in meteoric iron (Cañon Diablo [2]) and their manufacture by Moissan through the intervention of that metal.

Two suggestive discoveries must next be mentioned, of which, however, I was ignorant till within the last few months. A diamond had been obtained in 1892 embedded in a garnet (pyrope); and in another specimen no less than six diamonds occurred closely associated

[1] See *Geol. Mag.* 1897, p. 448.

[2] Another occurrence of diamond (not very pure) in a meteorite which fell at Novo Urei, Russia, Sept. 22, 1886, is mentioned by Professor Kuntz, *Eighteenth Ann. Report of the U.S. Geol. Survey*, Part V. p. 1195.

with, or indenting, or actually embedded in a fairly large, somewhat irregularly shaped pyrope. The one specimen came from Kimberley; the other from the Newlands Mines, West Griqualand, and it was found by Mr. G. Trubenbach, the managing director in England of the Company, during a visit to South Africa.

In these mines, as in the De Beers Mine,[1] rounded boulders occasionally occur in the diamond-bearing rock—the blue ground (soft or hard, as the case may be). Mr. Trubenbach brought some of these from the former locality to England, and a small diamond was then observed to be exposed on the surface of one of them; the boulder was broken and others were disclosed. One fragment was sent to Sir W. Crookes, to obtain the benefit of his opinion, and he showed it to me. Though I saw it by artificial light, I felt certain that the rock was not any variety of the breccia, but a true eclogite, and expressed that opinion. He most kindly asked me to examine the rock, and obtained from the directors permission for me to cut off as much as I thought necessary for a satisfactory investigation. I am deeply indebted to him for this kindness, and to Mr. Trubenbach for aiding me with other specimens from the mines and responding so willingly to my inquiries. An account of my examination of the whole series was communicated to the Royal Society on 1st June,[2] and the following are the principal results:—

The boulders of eclogite were six in number, but all prior to fracture had been well rounded. Stones of similar shapes might readily be found in the bed of an Alpine torrent after a course of several miles — in other words, I am sure they are water-worn. Three are of one species of eclogite, and three of another; two of the former being known to contain diamonds. That in which this mineral was first discovered is apparently from a quarter to a third of an ellipsoidal boulder, its axial measurements being roughly 4 in. × 3 in. × 2 in. The other specimen, probably about a quarter of the original, measured in the same way about $5\frac{1}{2}$ in. × 5 in. × $3\frac{1}{4}$ in. The outer surface of the former specimen is smooth; the pyropes[3] barely, if at all, projecting. So it has been in the other, but the surface now is slightly corroded. Near the exterior the pyropes, as is often the case, are covered by a dark outer film, thicker than the thumb-nail, but this is hardly perceptible near the centre.

The first-named specimen is comparatively rich in diamonds. Two are visible on the smooth outer surface, a third on one of the fractured faces, and seven on the other, but two of these (partially

[1] The occurrence of boulders in the blue ground of this mine (among them granite and eclogite) was mentioned so long ago as 1893 by A. W. Stelzner, *Sitzungber. u. Abhandl. der Isis*, Dresden, 1893, p. 71.

[2] *Proc. Roy. Soc. London*, 1899.

[3] I follow previous writers in applying that name to the red garnet of this rock and the washings. Its accuracy is confirmed by the fact that magnesia-mica is so abundant in the kelyphite rim.

covered by matrix) possibly may be in reality a twin;[1] five are exposed within a space about three-quarters of an inch square, three of them apparently in linear contact. These diamonds are octahedra (stepped faces), with an excellent lustre, perfectly colourless and clear. They vary in diameter from nearly 0·15 inch to 0·05 inch, and all apparently are embedded in the green part of the rock. In the second specimen only one diamond is visible, and this has been exposed by a slight flaking away from the outer surface. It is in all respects similar to those just mentioned. Each of these boulders, on microscopic examination, is found to be holocrystalline and to consist almost entirely of pyrope and a chrome-diopside. In a thin slice the former mineral is a light tawny red colour, is generally clear, but is much and irregularly cracked, and is occasionally traversed by wavy bands of minute enclosures, one set being branching and root-like, probably cavities, the other filmy, apparently a variety of brown mica, and indicative of incipient decomposition. The "skin" enveloping many of the garnets, especially towards the exterior of the boulder, is mainly composed of a mica of the biotite group, which in the latter case appears to be associated with a chlorite (by passage) and perhaps with a little fibrous hornblende. It is, in fact, a variety of the kelyphite rim, to which attention has often been called, but the radial structure is less marked than usual (so far as my experience goes), the mica flakes showing a tendency to parallelism. The chrome-diopside is the mineral described under that name by Professor Lewis; by others as omphacite or sahlite. In these slices it is a pale, dullish green colour, inclining to olive. The crystals are sometimes partially converted (at the exterior and along cracks) into a mineral, generally in minute matted fibres, but occasionally in grains large enough to show cleavage; these give the extinction of hornblende, and are no doubt the result of secondary change. The unaltered pyroxene shows one strongly marked cleavage (not so close as is usual in diallage), and a second less developed, sometimes almost at right angles to it. The former, as already noticed by Professor Lewis, is parallel to the clinopinacoid, and by measuring some flakes I obtained extinction angles up to quite 35°.[2] This diopside occasionally encloses a small rounded spot, consisting apparently of a serpentinous mineral, much blackened by opacite. I presume that a very few small grains of a ferriferous olivine were originally present, being among the first minerals to separate from the magma. In one of my slices the brown mica attains a larger size (about 0·03 inch in diameter) than at the margin of a garnet (from which it is dissociated), and exhibits a fairly idiomorphic outline (hexagonal prism). In this

[1] The point, of course, could easily be settled, but as it is unimportant I have preferred to leave things as they were.

[2] Professor Lewis obtained an angle of 39°. My measurements were rough, intended only for identification of the mineral.

case it is generally associated with a little calcite, and in one place with a radiating acicular mineral, probably a zeolite; in another the calcite is mixed with a serpentinous mineral. Larger grains of iron oxide appear to be wanting, and I have not observed zircon or spinel, or even rutile or pseudobrookite. Some of them might turn up, as a diamond might do, if more slices were cut,[1] but obviously they are not at all common. The second boulder corresponds so closely in mineral composition with that just described that a separate description is needless. I have also examined a fragment from a third rounded boulder, which when perfect must have been about a foot in diameter. The rock is practically identical with that of the other two boulders, but no diamonds are visible.

Three boulders, apparently without diamonds, represent another variety or species of eclogite. One is a fragment measuring about 7 in. × $4\frac{3}{4}$ in. × $3\frac{1}{2}$ in.; another an unbroken boulder, the girth of which, measured in three directions at right angles, is approximately $20\frac{1}{2}$ in. × $19\frac{1}{2}$ in. × $17\frac{1}{2}$ in.; and the third is a fragment about 3 in. × $2\frac{1}{4}$ in. × 2 in. In all these the outer surface is rather more decomposed than in the three described above, and the same appears true of the rock throughout. It obviously consists of three principal constituents, with a few scattered flakes of a brownish mica. Two of them, the pyrope and the diopside, do not differ from those described above, except that the former is slightly pinker in colour; the third constituent is an altered enstatite. The mica is only moderately pleochroic, resembling phlogopite; a small grain or two of serpentinised olivine (as before) may be present. Apparently the minerals have formed in the following order: (*a*) pyrope, (*b*) diopside, (*c*) mica, (*d*) enstatite. I had slices cut only from the first specimen, as I preferred to leave the second intact, and the third was more decomposed than the others. This rock obviously is closely related to the normal eclogites and to the eulysites—differing from the one in the conspicuous presence of a rhombic pyroxene; from the other in containing that mineral instead of olivine. If a special name be required I should propose Newlandite, but personally should be satisfied with enstatite-eclogite, for I prefer to call attention to relationships rather than to distinctions.

In connection with this rock an interesting specimen may be noticed, which was obtained from the blue ground. It is an irregular fragment between three or four inches long, consisting of crystals of a greyish-green rhombic pyroxene, in which one cleavage is strongly developed, but with a barely metalloidal lustre. They are approximately an inch in diameter, and between them small pyropes are rather irregularly interspersed. As I was reluctant to injure the specimen by cutting off a slice, I removed a few small flakes, which on examination with convergent light proved the mineral to belong to

[1] Five were made from the first boulder, three from the second, two from the third.

the bastite group, and I have no doubt it is the one present in the boulders just mentioned. The specimen accordingly represents a very coarse garnet-bearing bastitite.[1]

One more boulder still remains, though it requires only a passing notice. It is a compact greenish rock with spots of a light-coloured mineral. This proves on examination to be a rather felspathic diabase, with amygdales consisting chiefly of calcite, with chlorite, and a few small groups of zeolite.

These diamantiferous plots in West Griqualand, though on a smaller scale than at the older mines near Kimberley, occur in a similar way, and are formed of a rock practically identical. Those now being worked are three in number, two at least of them being connected by a line of fissure. The rock has now been proved, and galleries have been driven to a depth of over 300 feet, and the boulders above mentioned were found at various levels down to this from nearly 100 feet. A section obtained just south of the middle "pipe" is interesting. Here a gallery was driven between two walls of diabase (? dykes) about four yards apart, and in the interval were four ribs of blue ground, parted by country rock, which is a grey mudstone, sometimes pebbly. The total amount of the two was nearly the same, but the thinnest rib of "blue" (very decomposed) was about an inch in width, while the thickest was rather under four feet. It is strange that the characteristic "breccia" (though rather a finer variety than usual) should have penetrated into so narrow a fissure.[2] The principal areas, however, appear to be "blow-holes," formed in the same way as parasitic cones along a crack on the flank of a volcano.

Thus the diamond has been found to be a constituent of an eclogite, and the parent rock occurs as boulders in the ordinary diamantiferous material (blue ground). I have no hesitation in claiming this coarsely holocrystalline eclogite as an igneous rock, though I am aware that some uncertainty has been expressed on this point; but, as it happens, I have had several opportunities of studying eclogites, not only under the microscope, but also in the field, and am convinced that they are as truly igneous rocks as granites, syenites, or diorites. They are, indeed, rather closely allied with the last named, perhaps also with certain dolerites. The relationship may be expressed by the homely direction: "Put some salt into the magma of an ordinary eclogite and it will crystallise as one of the less acid diorites."

The diamond then is shown to be an accidental constituent of the

[1] Pyroxenites (diallagite, bastitite, etc.) not unfrequently run very coarse, but (so far as I happen to have seen) in rather thin dykes or veins. See *Quart. Jour. Geol. Soc.* vol. lv. (1899), p. 290.

[2] It will be remembered that the Kimberlite of Elliot County, Kentucky, appears to occupy a branching fissure (Lewis, "Genesis and Matrix of the Diamond," p. 64). As this section was obtained in a gallery at a depth of 300 feet it may possibly be misleading, and some of the blocks of mudstone may not be *in situ*, but only great fragments which have fallen into the fissure.

eclogite, as a zircon is of a granite or syenite. It may prove, however, not to be restricted to this one species of rock. I see no reason why it should not also occur in the enstatite-eclogite already described; while the fact that at Kimberley, if not at Newlands, olivine is abundant in the diamantiferous blue ground suggests the possibility that the diamond may also be a constituent of a peridotite. In fact, though I was unable to accept my late friend Professor Carvill Lewis's view that the Kimberlite was an altered peridotite, I fully expected that sooner or later it would be traced back to some very basic rock, probably to a peridotite. The diamond hitherto has only been proved to occur in meteoric iron[1] (Cañon Diablo), and it was made artificially by Professor Moissan by the intervention of that metal. Indeed, on *à priori* grounds I should have expected to find it in a rock less acid than an eclogite. I venture, accordingly, to suggest that the crystallisation of the carbon may possibly have occurred in some very basic magma which was afterwards invaded by one more acid, the eclogite being the result of the mixture. This, however, is a speculation; the fact, I think, cannot be disputed that the diamond has been traced back to an igneous rock (eclogite) and was not formed in the " blue " (Kimberlite).

The boulders described above appear to me truly water-worn; so also are not a few of the smaller fragments. I suspected this some time ago when examining a parcel of " washings " from the De Beers Mines (where also boulders have occurred), but those sent to me from Newlands have placed it beyond doubt; half a small pebble of eclogite is present, while many of the minerals are so well rounded that the darker kinds could only be determined by fracture. But if this be so, if many of the constituents are water-worn, how can the so-called Kimberlite be an altered porphyritic peridotite? We are compelled to regard it as a clastic rock, formed by explosions, which have mingled the shattered constituents of the coarsely crystalline floor with materials derived from the overlying sediments. The comparative abundance of diamonds in the blue ground suggests that they are fairly common in some members at least of the holocrystalline series. Hence it may be possible, by carefully observing the larger minerals found with diamonds, to infer which of them are really its associates. At present, garnet, chrome-diopside, and perhaps iron oxides, can alone be named, but I fully anticipate other pyroxenes and olivine to be added.

Hence, as the blue ground is not an altered peridotite, the name Kimberlite must be removed from the list of that group, and must disappear from science, unless it be retained for this peculiar breccia in which the diamond very commonly is an accidental constituent. The mode of occurrence, structure, and contents of this breccia suggest that it is the result of some kind of volcanic action, but the general absence of scoria makes it probable that the explosions were due to accumulated steam, and were thus of an exceptional character.

[1] The Novo Urei meteorite, however, is said to contain some ferro-magnesian minerals.

Discharges of lava occurred during the Karoo period and probably afterwards (for both the pipes and the surrounding sedimentary rocks are pierced by dykes), while the marked changes in the matrix of the blue ground (what has been one of the great difficulties in determining its real nature) suggest that for a long time it was acted upon by water at a high temperature. Thus the volcanoes did not go beyond the solfataric stage. They occur over a rather extensive district and are fairly numerous—comparable, in fact, with the volcanic necks of Fifeshire.

The diamantiferous boulders obviously have no connection with any existing alluvia. Probably they have come from a conglomerate at the base of the sedimentary series, resting directly on the crystalline floor. Thus far we have no means of determining what the age of the latter may be, but the Dwyka conglomerate of South African geologists—generally assigned to the Permian system — very probably extends beneath the Karoo beds of the diamantiferous region, and may repose on the crystalline floor. On that point, however, we must await further evidence; suffice it to say that the genesis of the diamond in South Africa was not a phenomenon of Mesozoic or later times, but must be yet more ancient.

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The Scope of Natural Selection.

Continued from page 129.

By J. Lionel Tayler.

The Primitive Characteristics of Protoplasm.

In this section I wish to briefly recapitulate a few well-known facts and generalisations, which appear to me to lead to the conclusion that natural selection acting on variations has been the sole means of producing divergence and evolution in the organic world, that protoplasm is never really modifiable, although it may be and has been adapted to a marvellous degree.

In the evolution of organisms certain generalisations have been shown to be in the main true. From the lower to the higher forms organisation tends to grow more complex and also more specialised; this development consists in a qualitative and a quantitative change. In estimating the value of any theory which claims to be able to largely explain the process of evolution this quantitative, as well as the qualitative, change must be kept in mind. If a study of the lower forms of life leads to the conclusion that even here elimination brings about adaptation, and that there is little or no evidence for modification of structure, while when we compare the higher and lower forms we find that the differences are very largely due to an increase in complexity, and that the qualitative difference is merely a further development or accentuation in the more advanced organism of a property which is always present in the less advanced, then it will be evident that the facts are largely in favour of a purely selectionist theory of evolution. That a study of the facts does lead to such a conclusion I shall now endeavour to demonstrate.

In the lowest forms of life we are confronted with a kind of substance (protoplasm) which manifests certain peculiarities which appear at first to sharply distinguish it from inorganic material. Protoplasm from its commencement, as far as we are able to examine it, appears to exist in two more or less distinct forms; these forms are not sharply marked off, but more or less shade into each other, but still are sufficiently clear and distinct to have led apparently to widely

different results. These two forms have developed on their separate lines and have resulted in the most important divisions of organic life, the animal and vegetable kingdoms; and the most marked difference between these two kinds of protoplasm appears to lie in the fact that one has to exist on comparatively complex foods, the other on comparatively simple. Excluding this and other differences, for the moment, from consideration, there remain three peculiarities which distinguish protoplasm from inorganic material:—(1) It is extremely complex in structure; (2) it is remarkably unstable; and (3) it has the power, when placed under suitable conditions, of building up from its environment material similar to or identical with its own.

Lewes, Spencer, and, in a crude unscientific form, many early writers, have noticed certain resemblances between some kinds of dead and living material; these resemblances have steadily multiplied in number, while they have become far more forcible in character during the last forty to fifty years, so that many, perhaps most, scientists are beginning to assume, consciously or unconsciously, that purely physical and chemical causes are or soon will be sufficient to explain the lower and possibly also the higher forms of life.[1] Let us take first the peculiarities of protoplasm which are apparently most allied to chemical and physical phenomena, its extreme instability and complexity. Making a general statement of the characteristics of the chemical elements, it appears that they may be grouped into three more or less ill-defined divisions—those with marked affinities, others with very ill-marked tendencies, and a third intermediary division. Stability is usually associated in chemistry with simple molecular structure; satisfied affinities and compounds are generally stable when they are made up of elements which exhibit strong mutual affinities, combined in such a way that each tendency is more or less completely balanced by others. The more perfectly the elements are brought into contact, the more combination of these elements is accelerated, and, finally, there is an evolution of energy whenever the less stable passes into the more stable.

Chemical instability, on the other hand, is associated with weak affinities, great complexity, and a combination of elements in a form which by readjustment might lead to the formation of simpler and more stable compounds. As there is always an evolution of energy when the less stable passes into the more stable, there is manifestly a storage of potential energy in the unstable forms. The instability and complexity of protoplasm is therefore really not a difference from, but a resemblance to, non-living substances, because its instability and complexity apparently exist under similar, though accentuated, conditions to those cases where the complexity and instability is purely chemical. The distinctive characteristic of living

[1] Verworn in his "General Physiology" gives a fairly complete summary of this position.

as opposed to non-living substances therefore must be found, if it exist at all, in some other property of living matter, and it may possibly lie in the third feature that has been noticed, its power of maintaining a constant mass of unstable substance under conditions which appear to make for disintegration of the substance; and we notice in addition another fact, namely, that while life lasts a continuous series of chemical changes, at some periods less active, at others more, but never entirely ceasing, are always present. Now in this perpetual chemical change some energy is wasted, and passes off into the environment in the form of heat, motion, etc. How does the organism get sufficient extra energy, not merely to maintain but even to frequently increase its complex and unstable substance? The extra energy might obviously be obtained if the organism continually assimilated more complex and unstable food than the ultimate products into which this disintegrated protoplasm broke down. In confirmation of this position it is noteworthy that plant tissues which have reached a much lower point of evolution than animal, and whose tissue change is less active, require less complex food than animals. For synthesis energy is required, and this could be obtained as above from the food material; in addition it would be necessary to have a very slightly conducting substance, such as we have in protoplasm, to prevent energy from being too rapidly dissipated, while every chemical reaction must be extremely rarefied, as any marked evolution of energy would obviously lead to the destruction of the whole organism. The essentials for the physical aspect of protoplasmic life would therefore appear to be, a certain small but constant amount of surplus energy which leads to a very gradual substitution of the less complex into the more complex, and then the gradual breaking down of the more complex protoplasm thus formed, by equally gradual stages, into simpler products than those which had been utilised as food.

It seems, therefore, conceivable, supposing chemical and physical conditions to be favourable, that a purely chemical product might be found which would, if situated in a suitable medium, manifest synthetical and analytical changes without any additional force being required. As further movements somewhat analogous in character to the amoeboid have been shown to be obtainable by chemical and physical conditions alone, as in the experiments of Quincke, Bütschli, and others, and also the various phenomena associated with chemiotaxis, phagocytosis, etc., appear to lead to the same conclusions, it would seem that the earliest forms of life might be accounted for on an entirely physical basis.

In many forms of bacteria, almost all the above conditions are complied with; they do not include any special phenomena of movement, or show any marked reaction to stimuli. There is usually a special temperature at which they grow most perfectly, while below and above this their growth and metabolism tend to cease, and they

will only grow on or in certain media. From a purely chemical standpoint, there is therefore nothing in protoplasmic activity which suggests any new element; that bacteria thrive under certain conditions but not under others, being dependent on their powers of combination and subject to the laws of chemical change, is consequently easily explainable. It may, however, be urged that while it is true that bacteria are sometimes influenced by some slight alterations in their environment, they are often capable of standing great extremes in other directions, and in this respect do not resemble unstable and complex chemical compounds; even this difference, however, does not hold, since there are many chemically complex and unstable compounds which appear relatively stable under certain conditions while they are equally unstable under others. There are, therefore, a set of conditions associated with early primitive life, which, except for the phenomena of fission which Spencer has shown, is, like the other properties of early protoplasm, capable of a physical explanation—are all explainable by the laws of chemical change, osmosis, diffusion, etc.

There are, of course, many fallacies to which one is liable in dealing with such a question; thus the extreme minuteness of the organisms, and our necessarily imperfect knowledge of their life-history and structure make it probable that any present-day explanation will be incomplete.

I only wish to note that this resemblance is likely to be at least partially true. That this apparent closeness of connection between chemical change and bacterial metabolism may appear to future generations less close than it does to us is possible, still the increased knowledge of the higher organisms, the relation of food-supply to bodily exertion, the recent work on digestion, blood-supply, and tissue change, do not lead to a less but a more close chemical analogy; in any case the inference, as far as the present time is concerned, is in favour of a very close connection between the laws of chemistry and physics on the one hand, and the forms of vital activity on the other.

Now, as far as this inference has weight, it must tell against climatic modification in favour of climatic and inter-organismal adaptation, inasmuch as chemical elements have definite affinities, enter into definite combinations in fixed proportions; and as any alteration in a compound, however complex, must proceed along definite lines, it follows that each form or variety of protoplasm, in so far as it is chemical in nature, can only grow and keep active by being fed by *certain* foods which it can make use of, and by being under *certain* conditions more or less favourable to its organisation; and when a sufficient number of these favourable conditions are not present, the surplus energy of the organism must in time run down, and the organism will die because it cannot utilise other conditions.

At the commencement of this article I endeavoured to emphasise the importance of keeping in mind the fundamental distinctions

between accommodations which are the direct result of environmental influence, just as wood becomes altered in its composition by a sufficient amount of heat, and those other forms of accommodations which are the result of the organismal response to its environment, and I pointed out that only in the former set of conditions was it strictly correct to speak of acquired modifications, and further that this somatic responsiveness was not in the least discordant with the principle of selection—it would, in fact, aid selectional development making the process of evolution more rapid. Now just as the chemical analogy tells against climatic modification, and in favour of use-development or organismal response with elimination of the less responsive, so I hope to show in this concluding portion of the paper that every broad generalisation tells against climatic modification, and in favour of organismal response, and I shall endeavour to show that the somatic response becomes increasingly separated off from the germinal, not through any special isolation of the germinal products, but for precisely similar reasons as other organs have become separated, namely, by increasing specialisation and complexity of structure.[1] In this concluding portion, therefore, of the article, I wish to keep these distinctions constantly in view:—(1) The direct climatic response, an external influence or influences producing internal modifications; except in so far as these external forces are destructive, I believe this influence to be negligible. (2) The response of the organism whether it be uni- or multicellular to external conditions and alterations that will ensue through elimination of the less fitted and preservation of the more fitted, internal response to external conditions, and external elimination of the less responsive organisms. (3) The relation, if any, that the somatic response bears to germinal variability.

In considering the chief differences between plants and animals, we find certain more or less constant conditions which lead to the conclusion that protoplasm is not directly modifiable; thus a broad general difference is found between these two great divisions of the living world in the fact that vegetable organisms live on simpler foods than animal. The fact that the fungi and certain insectivorous plants form a partial exception to this rule, only increases the strength of the selectionist position, for, from the fact that the vast majority of the various forms of vegetable life do live on simpler foods than animal, we may infer that the difference in the structure of the protoplasm was not easily overcome, while the constancy of the character of the exceptions now that a change has been produced is almost positive proof that if organisms can be directly modified by climatic action it must be to a very slight degree. The same line of argument applies to the other differences observable between plants and animals. On the assumption that this difference of metabolism

[1] Lloyd Morgan, in his "Animal Life and Intelligence," has put forward a theory of reproductive specialisation to which I am greatly indebted.

is due to a structural difference existing in the protoplasm itself, that the assimilative power of an organism depends not on its environment but upon its structure, and that these structural peculiarities are never modifiable, although they may be adapted through elimination of unfit and less fit, and subsequent reproduction among the surviving favoured organisms, and repetition of this process until a better and better adapted organism is produced, we have an explanation which satisfactorily accounts for both the constancy and the variability of the many forms of plant life.

Again, the constancy of all low forms of life under varying conditions is often remarkable. In view of the fact that these unicellular organisms are not easy to keep under constant observation, that their reproductive power is often enormous, and that it is at present very difficult if not impossible to place them under test conditions to prove whether or no they are capable of being directly modified by changes in temperature, food, etc., it is worthy of note that the few recorded experiments have taken years and not months or weeks to induce any change in the organism, and this suggests elimination rather than direct modification as the main if not sole agent.

The science of bacteriology is surely strong presumptive evidence that no very rapid modification of form and habits is affected by altered conditions in these low forms of life; the constancy of the characters of diseases known to be produced by these forms of micro-organisms, and the fact that the bacteriologist can frequently tell by the form and behaviour of the bacillus, micrococcus, etc., what disease it will induce, and this in spite of the immense capabilities for modification under changed conditions, etc., that its habits afford, are all arguments against direct climatic accommodation.

Another point which appears to me to throw very considerable light on the subject is the behaviour that all organisms, as far as I know, without exception, exhibit towards their environment. Local conditions of light, heat, food-supply, do not appear to modify organisms in a certain definite manner as one would expect were direct climatic accommodation possible; on the contrary, the action of every organism to its environment, from the lowest to the highest, appears to be selective, the response of certain internal activities to outside conditions. Recent observations made on the phagocytes of the blood show that the determination of their movements is partly chemical, that they move away from some and towards other products; their action is selective. Plants living on the same soil do not make use of the same material, and it is perfectly extraordinary what minute quantities of a substance can be utilized if it be needed by the organism. Iodine and its selection from sea-water by some forms of sea-weed is a case in point. Precisely similar results occur in the animal kingdom. The same choice of food is manifested in different

animals choosing different foods, the same blood circulating in the body of one animal yet has different substances extracted from it by different tissues; wherever we look we see life display this selective action towards its environment; if the materials that supply its needs are not present, the organism dies. This constant and universal tendency in living tissue to select out of many substances its own particular foods is not favourable to any theory of direct climatic modification; it does, however, favour the principle of selective adaptation.

The phenomena grouped around reproduction, in so far as it consists in conjugation and sex differentiations, seem to me to be explainable only on the assumption that protoplasm is scarcely, if at all, climatically modifiable. The simplest form of reproduction is that of simple fission; the single celled organism in which it occurs splits into two or more divisions. Spencer has suggested that the reason for this division may be, that unless very exceptional conditions of growth arise, there will be a constant tendency for volume to increase relatively to surface, and consequently that a point would at last be reached when certain portions of the cell would be insufficiently nourished. To decrease bulk and increase surface division would be necessary; such a theory of fission formed on mechanical grounds offers no difficulty to selection or other theories.

But if the relation that bulk bears to surface determines fission, it follows that fission will be favoured, as we have seen, by poor food-supply and by rapid metabolism, while the opposite conditions will favour slow metabolism; under the first set of conditions a small rapidly dividing cell would be favoured, while conditions that favoured slow metabolism would produce a large cell. On any system of climatic inheritance, the structure and needs of the organism would be modified according to the environment, hence one can see no need for conjugation. On any hypothesis that relies mainly or wholly on selection, it is, on the contrary, easy to understand that union of two nearly allied individuals would tend to preserve the stability in so far as they were allied, and would promote variability on the unallied smaller portion; there would be as a result an increased number of possible variations to select from, and those organisms in which conjugation occurred would be more likely to survive under all conditions, as they would always tend to adapt more readily. A certain limited unlikeness in the two cells which entered into combination would be favoured by natural selection, in order to preserve this necessary variability. This unlikeness might be the beginning of sex differentiation. The fact that conjugation occurs at all, may be explained in part by the fact that all living tissue has a certain selective affinity (and in this it presents many analogies to non-living) for what it has need of; conjugation might be merely the satisfaction of an organismic need.

The fact that the male cell is in some cases attracted to the female by chemical products [1] is some confirmation of this view. Conjugation would thus be allied to the phenomena associated with assimilation.

So far, therefore, the evidence appears to be in favour of protoplasm not being at any period directly influenced by climatic conditions. Protoplasm everywhere exhibits a tendency to select its food from its environment, and when it is unable to obtain such food, or is subject to conditions of environment which are unsuitable, it appears not to be rapidly modified, but is apparently eliminated. Protoplasm manifests in its different forms considerable resemblance to the more complex non-living chemical products, and this, so far as the inference is justifiable, points to the conclusion that certain conditions are essential for its development, that different forms of protoplasm require different conditions of environment, and that when any organism is not in sufficient harmony with its surroundings it is unable to live and is therefore eliminated. The constancy of the differences of the early forms of life would seem also to lead to the conclusion that protoplasm is never, or at most with extreme difficulty, *directly* modified by external influences. Lastly, the facts associated with conjugation and sex differentiation are apparently only explainable on a pure or nearly pure selectionist hypothesis.

Turning to another aspect of the facts relating to life, we find that while very considerable specialisation may be developed in unicellular organisms, yet when these organisms multiply they do so with very little alteration of the mother plasm, reproduction consisting in the separation of a portion of this mother substance, this portion, whether small or large, becoming a separate organism.

In multicellular organisms, on the other hand, we see, besides this method of reproduction, another kind, which very early in biological evolution takes precedence over the more primitive method. The younger organism is developed from a structure that is not represented in the adult form, and the younger organism begins to closely resemble the older only after a period of development. In what respect is this latter kind of reproduction superior to the former? In the hydra we have an organism in which these two types co-exist. A new organism is sometimes developed as a simple out-growth of the mother substance, develops a mouth and tentacles, and with this new mode of obtaining nutriment gradually loses its connection with the parent organism and becomes independent. In other cases we find interstitial cells collecting into groups at different parts of the organism, in some of these groups the inner cells becoming slightly altered in shape, and developing thin, ribbon-shaped pieces of protoplasm or tails, by the aid of which they become capable of considerable powers of movement, and thus provided escape from the hydra into its surrounding medium. Other groups of cells undergo a different change, one

[1] Hertwig's work on "The Cell" gives a brief résumé of some of these cases.

cell, again occupying an internal position in the group, enlarges at the expense of the surrounding cells, and when it has attained a certain size ruptures from the capsule which surrounded it, extrudes two nuclear portions of its substance (polar bodies), and if one of the smaller active cells comes into contact, and fuses with it, it will commence a series of cell divisions accompanied by increasing growth, and develop into an adult hydra similar to its parent. This sexual mode of reproduction very rapidly supplants all other forms; it is probable, therefore, that there is some immediate advantage resulting to the organisms which reproduce in this way rather than by budding. The most obvious difference in these two methods is that there is a great reduction of tissue material, much less being required for this mode of development than the other; it is therefore less expensive to the parent organism. Apart from this there is the additional factor that it would be the most suitable for development, if direct climatic accommodation does not take place, owing to its being the best means of obtaining the requisite amount of variability. This reduction must presumably be largely quantitative and not qualitative, since we find that under very dissimilar conditions a complex hydra can be formed, provided portions of both ectoderm and entoderm are preserved.

Now, where this sexual mode of reproduction arises, we have to consider a new set of conditions; we find that each individual appears to go through a stage of development, maturity, and decay, and that during maturity the reproductive power of the whole organism is best developed.

Perhaps one of the most striking facts associated with the higher forms of life is that these three periods of growth, maturity, and decay in the whole organism do not correspond in time to similar periods in the several different parts of the organism in question. This fact appears to be universal in its application; how is it to be explained? Now, as I have already noted, the most marked difference between unicellular and multicellular reproduction consists in the fact that the latter develops chiefly by a quantitative evolution from a cell which is quantitatively undifferentiated, while the former reproduce by splitting off a portion of their structure, so that in most particulars, except size, the parent and the offspring are identical. Now one of the peculiarities of development and growth in one of the higher organisms is just this quantitative development, and we must assume that the morphological element is present, for it is inconceivable that actual differentiation of structure could arise without some structural difference for its starting-point. We are bound therefore to assume two positions as essential to development: (1) Some basis for the differences that are found in individual development which must be of a structural and not a physiological nature, whether we call them gemmules, physiological or morphological units, biophors or stirp; (2) that development consists largely in a reduplication of

parts which at the time of fertilisation are somehow or other qualitatively represented in the fertilised ovum.

In development every organism passes through a series of stages which are more or less proportional to its specialisation and complexity, and the definite stages are passed through in a definite order, the highest specialisations, except where definite atavisitic or degenerative phenomena intervene, always coming at the later periods of development. When decay sets in in the organism we not uncommonly find that this order is reversed, the higher being the first to disappear, just as they were the last to come. In the action of many drugs we see the same tendency; if their action is general, the highest nerve-centres go first, the lowest fail last. Now this sequence in development, since it is so universal, must serve some purpose. The very early stages of segmentation appear to be little else than quantitative in character, but later qualitative differentiation begins to be manifested. The study of life in recent years has shown conclusively what an enormously important part the various products of tissue metabolism exert over life; the toxic and anti-toxic theories in disease, phenomena associated with internal secretion, the influence of vegetable alkaloids on different animal tissues, etc., all go to show that tissue activity is very dependent on its surroundings for its activity. Some facts of embryology lead to the conclusion that some organs have an almost purely developmental significance, and are of little use to the developed organism. We know also that organs vary in their relative importance and size to the whole organism at different periods of its development. How are we to explain the cause of this atrophy of some organs while others are developing, except on the assumption of a chemical food sequence? If we assume that, with a growing specialisation, itself induced by the liberation of metabolic products in the preceding stages, there is a growing specialisation of ferments and other material necessary to a more developed organism, and as a consequence a growing specialisation of all food material, we shall have a theory in accordance with facts, and which can explain many otherwise incomprehensible phenomena. The more specialised the food products circulating in the organism, the less favourable the conditions for the more generalised tissues; hence the progressive development of some tissues, and atrophy of others, would be explainable.

The sequence in development would then be itself explainable, as the higher could only be developed from the lower through this sequence; hence the necessity of recapitulation of the ancestral types in development. Rudiments would on this theory disappear in proportion to the generalised character of the rudiment as compared with organismal specialisation, and this would apply to germinal and somatic development. On this theory the whole organism would continue specialising so long as the morphological elements allowed of further differentiation; when this limit of specialisation was reached the organism

would arrive at maturity, and, so long as each tissue remained proportionately active, health would result, but when this balance failed degeneration and disease would result.

We come now to the concluding question, the relation that germinal development bears to somatic.

As an organism reaches maturity, the phenomena associated with reproduction become manifest; this fact is practically universal, it holds good for multicellular and unicellular organisms alike, and for both the animal and vegetable kingdoms. In unicellular organisms, as we have seen, it is probable that there is a mechanical limit to the size of the cell, beyond which growth as a single cell becomes impossible; this growth limit will not be the same under all conditions, but must ultimately be reached in all forms of single-celled organisms.

In the metaphyta, under suitable conditions, there appears to be a nearly constant tendency to growth at any place where a breach of continuity is formed in a living tissue or tissues; in the lower forms of metazoa removal of a portion of tissue is nearly always followed by growth of the remaining, so that more or less complete repair results; in the higher animals, on the other hand, this local reparative process is much less complete, yet even here some attempt is always present.

The fact that removal of tissue tends to produce activity and growth at the seat of injury suggests that possibly some mechanical limit to growth is one of the causes of cessation of growth.

The inferences so far necessary to determine the relation that somatic development bears to germinal may now be summarised as follows. I have endeavoured to point out that facts do not favour direct climatic modification, and I accept the Neo-Darwinian conclusion and believe that there is very little evidence for the transmission of somatic responses. From a study of facts which have universal applications I have endeavoured to show (1) that growth and reproduction are in some way closely related; (2) that facts justify the inference that an increasingly complex food sequence prepares the way for morphological quantitative specialisation; (3) that some morphological explanation of heredity is necessary to explain the facts. Some such provisional theory as the following would, I believe, explain the facts of heredity, growth, decay, and certain facts which have reference to disease, better than previous theories:—

1. That there is a mechanical nutritional limit of growth for each cell, that this bulk limit varies according to physical conditions and food supply, but is reached sooner or later by all growing cells (Spencer). When this limit is reached, cell division takes place, which may be equal, as in fission, or unequal as in budding, etc.

2. Under conditions which demand variability of the organism, conjugation of similar organisms placed under similar conditions would be favourable for the attainment of this requisite variability. If protoplasm is never directly modified by climatic conditions, then the

best chances of survival and adaptation, either to old or to new conditions, would be through conjugation. Selection would therefore favour conjugation (Weismann).

3. If for some reason, possibly nutritional in origin, fission in an organism had not been quite complete, and the cells instead of separating had remained together, then as each new division reached maturity it would divide and the process of division would continue till interfered with by some outside condition, many different forms of these masses of cells would thus be produced, examples of which may be found in the different forms of sponges. Now, if for any reason a curved single layer of cells was formed, it would go on growing in all directions until it met other cells of the same collective cell colony; a multicellular growth limit would thus be reached. Now, assuming this growth capacity to remain constant, one of three things can happen. With a somewhat irregular hollow sphere of cells, it would be conceivable that: (1) a bending in at one of the weaker points, or (2) a bending out would occur, many cells being involved in this yielding; or (3) each cell might bud off a certain portion independently. Of the first or outward yielding, and the formation of buds, we have many examples occurring in nature, as, for example, bud development in the hydra; of the inward yielding, the passage from the blastoderm to the gastrula stage, through the process of invagination occurring in the development of many animals, affords an example of the second means of satisfying this growth tendency; while in the third case division of the individual cell, and separation from its parent tissue, occurs in the formation of red blood corpuscles in mammals, etc.

4. It is obvious that the general structure of the organism would be least disturbed by each individual cell throwing off buds, and therefore the more specialised the organic structure the less likelihood of those organisms that reproduced by any collective alteration of the the organism surviving. With growing specialisation each tissue will become less and less able to reproduce other than its own specialisation, hence reproduction will occur only when the buds from the requisite differentiations meet; now in the case of the hydra it appears to be only necessary to have representatives of two classes of cells, the ecto- and entoderm, and these thrown-off portions of cell structure would, when the requisite number met, owing to perhaps some stronger growth tendency, tend to push up the cells above them, and as the most likely place for the ectoderm and entoderm units to meet would be *between* these two layers, we should expect development to commence from this position. With increasing differentiation reproductive centres would tend more and more to be localised to one centre. Hence with increasing specialisation there would be progressively less power of local or somatic reproduction.

5. A special kind of organism survives for two reasons: (1)

because it is suited to its environment; (2) because it can reproduce similar organisms in sufficient number to maintain or increase its relative position in its environment. The more perfect the organism the less its chance of elimination, consequently so long as its reproductive power is successfully maintained it is to its advantage if it can reduce to a minimum the loss incurred by the organism in successful reproduction; it will follow, therefore, that the cells which throw off least reproductive material from the adult structure will require less nutriment, and therefore the collective organism will, other conditions equal, survive under competitive conditions. For this reason protoplasmic growth will be reduced as far as possible when beyond the needs of the organism, and the reproductive buds or units from each cell will tend to be reduced both in size and number. For these reasons it would obviously be of advantage if merely the morphological elements were extruded from the different cells,[1] and these when collected in the reproductive centre would form the material for the new individual.

6. As differentiation of reproductive function continued running a parallel course with other specialisations of structure, natural selection continuing to favour the best-formed individual and offspring that environments could allow, two tendencies would become manifest: (1) a tendency to reproductive economy, by which every unnecessary development would be eliminated so as to make reproduction a less and less expensive process to the organism; (2) owing to increased complexity, specialisation, and evolution of structure, reproduction would become a more and more delicate process, and would constantly have to be conducted with increasing care, and the stages of development of the organism would therefore become increasingly prolonged. The development of the individual, and the capacity of that individual when developed for competition with other individuals, would form two partly competing and partly complementary elements of race progress, and the resultant of the two would correspond to the line of progressive adaptation and development. With the increasing length of the period of development differentiation of sex becomes first an advantage and then a necessity.

7. A progressively specialised method of food supply will be required to keep pace with the other specialisations.

In applying these conceptions to the interpretation of phenomena, certain points must be specially emphasized:—

(*a*) Every important specialisation of structure must be represented.

(*b*) As, however, one of the causes of evolution of structure is quantitative complexity, it follows that every quantitative element need not be represented, but only the right

[1] In the extrusion of the polar bodies from the ovum, we may possibly have an instance of what on a smaller scale is universal among multicellular organisms.

proportions preserved between the various qualitative specialisations.

(*c*) Reproduction on this theory commences when full or nearly full development of a structure is reached, when its growth capacity is in excess of its demands; from this it will follow that the reproductive units will be collected in the reproductive organs in the order of their evolution.

(*d*) A progressively specialising food supply would determine the development and the atrophy of the different reproductive units.

(*e*) The later a specialisation was developed either in the history of the species or the individual the less chance of its obtaining a foothold in reproduction, and conversely these must be the first to be eliminated under stress conditions. It will follow from this that the effects of use and disuse, in so far as they are of a somatic nature, will be very little if at all transmitted to the germinal structures, since development, in so far as the major part of the organism is concerned, will be completed early.

The first advantage of a theory like the preceding is that it has no need for the supposition of any isolated germ structure, use-inheritance being largely negatived by specialisation. The relation of germinal to somatic development is on this theory understandable. It would account for recapitulation in development, not on the ground of a tendency in the organism to repeat certain ancestral characters, but simply as the necessary preparatory specialisations out of which the later ones are built.[1] It would divide all anomalies into—(1) those cases of faulty representation due to the missing of some prior stage in development, as in the case of cretins, where the morphological element is there but the means of developing it is not, or where deficiency of the element itself as possibly happens in the case of mongoloid idiots; (2) disproportionate representation (quantitative anomaly), leading to dichotomy, etc.; (3) under rare conditions the reappearance of real ancestral characters.

If therefore the recapitulation theory has a different meaning from that of ancestral repetition, and if most cases of so-called atavism can be explained on the assumption of incomplete development, if it is further borne in mind that given the power of segmentation then all that is chiefly required is a proportionate representation of germs, then the complexity of the germ plasm, although very great, need not be so inconceivably great as that which involves the representation of a large number of ancestral as well as all living characteristics. Normal sexual reproduction would on this theory be the right

[1] In a limited sense, however, these stages would represent the history of the individual ancestral line.

principle for selection to rely upon, since the male and female lines of heredity would be largely in harmony over the earlier stages of development, the tendency to vary being increased towards the later stages, thus the requisite stability and variability would be largely obtained. Finally, this theory involves no very great assumption; it is, when examined, very little more than a series of inferences drawn from peculiarities of life that appear to be nearly or completely universal in application, being dependent solely on the assumptions of mechanical and chemical limits to growth, the latter being no longer an assumption, but an established fact in some instances, on the innate capacity for growth, qualitative and quantitative specialisation, and upon the conclusion that protoplasm is never directly influenced by climatic conditions. The theory of co-incident variability and the non-inheritance of acquired responses would equally accord with this theory as with Weismann's, while it would account for those cases of modifications which have been effected during the early stages of development.

In conclusion, I have endeavoured to show reason for believing that the principle of selection, when rightly viewed, is the only theory which is capable of explaining the various phenomena in their entirety; that the properties existing in the lowest forms of life do afford sufficient material for natural selection to act upon, and therefore, until it can be shown that another theory is in more complete accordance with the facts, that natural selection must be regarded as the dominant factor of evolution.

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Suggestions upon the Origin of the Australian Flora.

By Spencer Moore, B.Sc., F.L.S.

Of all the problems which have engaged the attention of those biologists for whom questions relating to the distribution of life upon our globe have possessed special interest, none has appealed with more fascinating insistence than that one which concerns the stocking of Australia with its animal and vegetable inhabitants. Many are the memoirs wherein this subject is treated either as a whole or in some special and subsidiary aspect. The former method, the method adopted, for instance, with so much brilliancy by Mr. Wallace, is, of course, the more satisfactory one, inasmuch as the same general principles must—due regard being paid to special circumstances in their application to each individual case—have been operative in all departments of both kingdoms of nature. But although only the scantiest reference to zoological problems is made in the following pages, it is believed that the views maintained in them are in no way discordant with the ascertained facts and recognised deductions of zoology: indeed, were this not the case, the task I have set myself would be a hopeless one. But it is otherwise difficult enough, involving, as it does, rejection of views which have received such weighty advocacy, both here and on the Continent, as has raised or almost raised them into the rank of axioms of science.

Before explaining my ideas, however, it will be necessary to dwell for a time upon one theory to which general adhesion has been given, in my opinion, without sufficient warrant. Basing their conclusions to some extent on zoological data, and swayed by the bias imparted by those data, botanists have assumed that the Australian flora is of a lower and less specialised type than that of the northern hemisphere and the tropical regions. It exists to-day, we are told, simply because it has remained isolated from the great land areas of the Old World, and but for this, an exotic flora would have overrun the island-continent as certainly as, without the interposition of the ocean, the Marsupial and Monotrematous fauna would have disappeared before the inroads of higher Mammalia better adapted to the conditions of

existence. It is proposed now to throw the search-light of analysis upon this theory, with the object of ascertaining whether it rests on a real substantive basis or no.

The first point to be dealt with is the idea that species belonging to genera predominantly extra-Australian must necessarily have had their origin outside Australia, whither they have migrated, some inherent superiority possessed by them over forms truly endemic having enabled them to maintain themselves and gain ground in their new home. In this relation the two floras of special concern are the Scandinavian and the Indo-Malayan. "The Scandinavian asserts his privilege of ubiquity," writes Sir Joseph Hooker,[1] and the same botanist tells us he regards "the Indian plants in Australia to be as foreign to it, botanically, as the Scandinavian, and more so than the Antarctic."[2] Mr. Darwin[3] goes so far as to ascribe the "aggressive power" of the Scandinavian flora to the fact of that flora having been differentiated in the most extensive land-area of the globe, where competition has been most severe and long-continued. But the supposed long continuance of this competition traverses well-established geological data, which teach us that the undisputed sway of this flora over Northern Europe and Asia dates only from post-Miocene times; while as regards the nature of the competition, who can possibly say that European plants have been subjected to greater stress than those of the old and new world tropics, of South Africa, or of Australia itself? Mr. Wallace[4] has no doubt about this Scandinavian predominance, though he is neutral as regards Mr. Darwin's explanation of it; and Professor Tate,[5] who has recorded his recent experiences in Central Australia in an ingenious and suggestive memoir, finds warrant for the belief that an exotic vegetation is there gaining the upper hand over the indigenous flora. In the face of such authority, and more could be cited were it necessary,[6] it will, I hope, be believed that the attempt here made to maintain a contrary opinion is undertaken in a spirit of diffidence, and without the slightest desire of asserting a rebellious originality.

It is not to be doubted that during past ages facilities have existed for the transport of northern forms through the tropical highlands into southern countries and *vice versâ*. Whether this migration has been largely favoured by cooling of the tropics during glacial periods, or whether, as is perhaps more plausible, it has been in great part due to transport by ordinary agencies such as the winds, the movements of birds, etc., is not a question we have here to discuss. Under the first supposition it is difficult to understand, as Sir Joseph Hooker has pointed out,[7] how tropical species could have survived, though, as the

1 "Flora of Tasmania," Introd. Essay, p. ciii.
2 *Loc. cit.*
3 "Origin of Species," ed. vi. p. 340.
4 "Island Life," p. 511.
5 "Botany of the Horn Expedition," p. 120.
6 These remarks being of the nature of suggestions merely, I have refrained from quoting bibliography except when that course seemed unavoidable.
7 *Trans. Linn. Soc.* xxii. p. 259.

supposition deals with *secular* changes, that is with conditions entirely outside the limited range of our experience, speculations on the subject cannot be said to be quite conclusive. The fact we have to recognise is that migration has taken place, whatever may have been the agency or agencies whereby it was effected.

Now, the most successful migrants should be herbs, for the seeds of herbaceous annuals falling upon favourable soil will rapidly germinate, and the seedlings will run through their life-history in a season lasting only a few weeks. So, too, free-seeding biennials and perennials will take possession of an unoccupied area, and produce offspring soon ready in their turn to extend the range of the species whenever occasion offers. Far otherwise is it with shrubs and trees, which require several years before they bear seeds. Competition, too, between trees and shrubs will be much keener than between herbs; for each of the former must have a considerable space for the support of its assimilating organs; their area also will be limited by such a condition as depth of soil, and they are liable to destruction by storms. Moreover, unoccupied spaces are left between them, and here herbs can flourish. And when it is remembered that the stepping-stones, as it were, which have been made use of in the transport of plants across the tropics—the mountain-ranges, that is to say—are especially adapted to herbs, many of them living above the regions of trees and shrubs, we see how great an advantage in migration has been enjoyed by herbaceous plants over woody ones.

We come now to the next point, which is, that while in the north part of the northern hemisphere the proportion of herbs to shrubs and trees is so large as to justify our calling this portion of the globe a herbaceous zone, the south part of the southern hemisphere, where it is not occupied by the ocean or by glaciated land, comes for the most part within what I shall term a dendritic zone, meaning by this a zone where woody vegetation predominates over herbaceous. New Zealand, temperate Australia, South Africa, the greater part of extra-tropical South America are all dendritic lands. Given, therefore, opportunities of transport from either hemisphere into the other under conditions similar or approximately similar to those now existing, and herbs being better adapted to transport than woody plants, the probabilities are that the preponderating trend of migration will be from north to south, and this without any inherent superiority in the northern flora due to competition in the largest land-area of the world, or to any other cause whatsoever.

"But," one fancies an objector saying, "consider how large a number of northern species have passed over into the southern hemisphere, and how few and far between, and even then how limited in their range north of the Equator, are the southern types which have succeeded in gaining a foothold in the northern hemisphere." But this statement assumes our possession of more knowledge than is at

our command. Is it so certain that all the species of the Scandinavian flora have originated in the northern hemisphere? Sir Joseph Hooker, it is true, guards himself verbally from this assumption by enumerating certain genera found south as well as north of the tropics as "eminently characteristic" of the northern flora. But the inference remains nevertheless, and we have only to consider the case of the Marsupials and Monotremes—orders "eminently characteristic" of Australia, but which we know upon zoological evidence not to have originated there—we have only to consider this case to see how unjustified is the inference, and how liable we may be, by adopting it, to fall into complete error. And it will be well here to deal, by way of example, with a few genera usually regarded as of northern origin, but which, it is maintained, may have originated in the southern hemisphere. There is *Senecio*, for instance, a genus strongly represented in extra-tropical South America (Philippi enumerates no less than 117 species as members of the Chilian flora alone) and in South Africa, and less strongly in Australia and New Zealand. The general view about such a case as this is that the areas just mentioned are isolated from each other, while each is in complete or almost complete connection with the great northern continent; hence the probability is that they were stocked from the latter. But, given a means whereby the species of *Senecio* could pass from north to south, there is no inherent reason why they might not have migrated in the opposite direction, say, for example, from South Africa by way of Eastern Asia into America on the one hand, and *via* what is now the Indian Archipelago into Australia on the other, and certain affinities between the floras of South Africa and Australia seem to show that some such migration has actually occurred. Again, take *Drosera*, a genus which, from the bias of early association, is usually regarded as having originated in the northern hemisphere, but which, in point of numbers and of differentiation, is far better represented south of the Equator than north of it, and very strongly in Australia itself. Then there is *Veronica*, with 15 Australian and no less than 40 New Zealand species, with 18 species in India, chiefly the Himalayas, about 20 species in North America, and not quite so many in China. Out of a total of some 160 species for the whole world rather more than one-third are natives of Australia or New Zealand or both. *Aster*, too, is a case in point, for though the Australian *Olearia* and the South African *Felicia* have been separated from it, and may still be kept up for convenience sake, in no essential respect do they differ from *Aster*, of which over 200 species are North American, while there are about 50 species of *Felicia* and nearly 70 species of *Olearia* in Australia and 20 in New Zealand. Now *Aster* is a genus eminently characteristic of the nearctic portion of the great northern land-mass, but if it had a northern origin, why is it so rare in Europe, a region where many of its species have become naturalised and are able to maintain themselves? Why may

not the genus have originated in Australia and passed thence *via* Eastern Asia, where it is represented by several species, into North America? Only on the hypothesis that a genus must have arisen in a larger area and that its presence in a smaller area must be due to migration, which is a mere begging of the question, can the possibility of a southern origin for *Aster* be denied. Mention may be made, too, of *Bassia*, in Mueller's sense of the term, that is, as comprising *Chenolea*, *Sclerolaena*, *Anisacantha*, *Threlkeldia*, and part of *Kochia* as understood by Bentham. Of these *Sclerolaena*, *Anisacantha*, and *Threlkeldia* are endemic in Australia, and the two species of *Kochia*, referred to *Bassia* by Mueller, are also endemic there, *Chenolea* alone being extra-Australian with nearly one-third of its species restricted to the island-continent. Yet *Bassia* is held by Professor Tate to be a genus exotic to Australia! So, too, *Kochia* proper has 19 Australian species, all endemic, leaving only 13 to be shared between South Europe, temperate Asia, North and South Africa, India, and North-West America; and when we remember that several peculiar genera allied to *Kochia* are exclusively Australian, is there anything extravagant in the opinion that probabilities point to this genus as having originated in Australia? And what shall we say of *Atriplex*, of which many species are Australian, and some of them extraordinarily abundant in individuals? The evidence for a southern origin of such genera as *Ranunculus* and *Clematis*, *Myosurus* and *Samolus* is not so strong; but when we come to aquatics, such as *Callitriche* and *Ceratophyllum* and *Potamogeton*, all very extensively distributed, I do not see upon what grounds the possibility of a southern origin for some of them can be scouted, and it must not be forgotten that *Myriophyllum* belongs to an order reaching its maximum of species in Australia. Then take the Grasses, an order very abundant in both hemispheres. Why may not such genera as *Deyeuxia*, *Hierochloa*, *Stipa*, and *Eragrostis*, to mention a few only, have originated in some southern land or lands, and migrated thence to their present homes in the north?

These are merely a few cases mentioned by way of example: by no means do they exhaust the list of genera for the southern origin of which there is at least some probability. But it may be objected that most of the genera cited above are not found in antarctic lands, and how, it will be asked, is their absence explained if they had a southern origin? I reply that, for all we know to the contrary, antarctic lands may, at some former time, have supported many supposed northern genera now not found there. This traverses Mr. Darwin's opinion when he says:[1] "I am inclined to look in the southern as in the northern hemisphere to a former and warmer period, before the commencement of the last glacial period, when the antarctic lands, now covered with ice, supported a highly peculiar and isolated flora." But with all deference to Mr. Darwin, why should the pre-glacial antarctic

[1] "Origin of Species," 6th ed. p. 341.

flora necessarily have been peculiar and isolated? If there is one point on which students of biological geography are agreed it is this, that the antarctic continent must formerly have extended considerably farther north than it does now, an extension which permitted the migration of certain animal forms from South America to New Zealand, and must equally have allowed the southward migration of South American and New Zealand plants. This stocking of the antarctic continent may have occurred comparatively early in Tertiary times, and so long as glaciation did not supervene, a large and by no means peculiar or isolated flora may have flourished in the antarctic continent. But now, communication with lands lying to the north being cut off, if a glacial period occurred, the result in the southern hemisphere would be very different from one in the northern, for while in the latter there would be nothing to hinder the southward migration of plants, their escape from the antarctic continent would be cut off by the ocean, and since all antarctic lands must have been covered with an ice-cap during a glacial period, all, or almost all, but the lowliest organisms must necessarily have perished. Obviously the nature of the flora of the antarctic continent previous to the last glacial period must have depended upon the occurrence or no of a glacial period or of glacial periods intercalated between the last of such periods and the stocking of the continent when it was in connection or close relation with lands to the north. If no such period intervened, then the flora must have consisted of a mixture of South American, New Zealand, and possibly to some extent of Australian types, or of descendants from such, together with endemic genera, of which many, for all that we know, may have been identical with genera characteristic of northern lands. But if a glacial period was intercalated, and that after the connecting lands to the northward had disappeared beneath the waves, then the flora of the antarctic continent during the subsequent warm period must have been closely similar to that of other antarctic lands, since it would have been derived from the same source or sources; while if the connection with lands to the north was still open at the commencement of the intercalated glacial period or periods, the antarctic flora would have migrated northward, and, the connection being still maintained, would have advanced southward on the return of warmer conditions, so that it would have borne approximately the same *facies* after as before the glaciation of the continent. If this reasoning be sound, therefore, in no event does it seem likely that the antarctic flora could have been in any special sense isolated and highly peculiar.

As an instance of the way in which the brief—if the term may be allowed without offence—for the predominance of the northern flora has been handled, I shall cite the assumption that glaciation first affected the northern hemisphere. Let us hear Mr. Darwin. After alluding to the southward migration of species when glacial conditions obtained

in the north, "then," he says, "in the regular course of events the southern hemisphere would in its turn be subject to a severe glacial period, with the northern hemisphere rendered warmer; and then the southern temperate forms would invade the equatorial lowlands. The northern forms which had before been left on the mountains would now descend and mingle with the southern forms. These latter, when the warmth returned, would return to their former homes, leaving some few species on the mountains, and carrying southward with them some of the north temperate forms which had descended from their mountain fastnesses. Thus we should have some few species identically the same in the northern and southern temperate zones and on the mountains of the intermediate tropical regions." [1] Now we have as much right to assume that glaciation first affected the southern hemisphere; and a clear idea of the result will be gained if the reader will substitute "south" for "north," and *vice versâ* in the above admirable quotation. Yet what a different idea of the trend of migration it gives us!

But my imaginary opponent now proposes to crush me with an argument he has carefully held in reserve. "Consider," he exclaims, "the evidence furnished by introduced plants. Wherever man settles, his footsteps are dogged by Scandinavian species, which rapidly establish themselves in their new home and at the expense of the indigenous vegetation; how could this happen unless there is some potency inherent in northern forms over and above that possessed by the southern flora?" While admitting that a considerable number of northern plants have become naturalised in southern lands, it must not be forgotten that some, though a far smaller number, of southern species have gained a foothold north of the equator. But in order to estimate properly the value of this preponderant naturalisation of northern forms, we must not be contented, although even Mr. Darwin seems to have been contented, with merely drawing up lists of the colonists of either hemisphere; before ascribing any aggressive power to the northern flora, we must ascertain that no other explanation of the facts is possible. And firstly, we note, and it is a matter of great importance, that almost all the plants naturalised in southern lands are herbaceous. We may take as an example Sir Joseph Hooker's list of introductions into New Zealand.[2] It amounts to 170 species, of which fully half are annuals, thirteen are biennials, and over fifty of the remainder, although perennial, are herbaceous. Now what has happened in New Zealand since the first batch of colonists landed on its shores? The densely clothed forest-lands have been cleared to make room for the herbaceous vegetation on which man depends for his sustenance; in other words, a dendritic zone has been artificially converted into a herbaceous one. And not this only, but the seeds

[1] "Origin of Species," 6th ed. p. 339.
[2] "Handbook New Zealand Flora," p. 757.

of these economic plants have been introduced from the north, and at the same time the seeds of other plants accustomed from time immemorial to flourish in association with them, as well as the seeds of species which have been allowed, for the sake of old recollections, to obtain a foothold in the new homes of the race. We have seen how advantageous it is for a migrating species to be herbaceous, and a still greater advantage should obtain where migration has been so effectually assisted by human effort. Then again, a point we ought to have information about, for it has material bearing on the case, is whether the indigenous herbaceous vegetation has benefited by the introduced changes.[1]

But the case becomes still stronger when we take Australia into consideration. The fierce droughts experienced by so large a part of that country have brought about the survival of a vegetation to a very large extent xerophilous. Now there is one peculiar feature about all desert countries except the very dryest, a feature necessarily tending to favour the spread of any herbaceous vegetation of which the seeds may chance to be introduced into them, namely, that at least during some part of the year there are always places where water is apt to collect, and where the ground will remain moist during the short time while the life-history of a herb is being enacted.[2] This is simply what one sees in the interior of Western Australia. For a period long enough to ensure the maturation of their seeds, introduced plants enjoy, in normal seasons, conditions precisely similar to those obtaining in their native habitats. But no sooner does the sun gain in power, and the ground become dry and warm, than these herbs completely disappear; they show, in fact, none of that capacity for adapting themselves to their altered surroundings which we should expect members of an "aggressive" flora to possess. This is, however, not the only advantage "Scandinavian" species enjoy when introduced into a country with a dry climate such as Australia. If one or more seasons of drought supervene, what happens? Considerable though varying power of latency is possessed by the great majority of seeds, and under these circumstances the introduced herb is in precisely the same position as the indigenous, both having to await a favourable season in order that their seeds may germinate. Contrast this now with the fate awaiting seeds of dry southern climates introduced into a country with a climate like ours. A short spell of warmth sets in,

[1] Authoritative information on this subject has recently come to hand (*vide* T. Kirk, Presidential Address to the Wellington Philosophical Society, 1895; abstracted in *Journ. of Botany*, 1896, p. 338). From this it is clear that in some cases indigenous species have benefited by changes due to human agency.

[2] The conditions in Australia are specially favourable to the introduction of cold temperate herbs, inasmuch as it is only when the temperature is low, that is, when the conditions approximate to those of the summer of Northern Europe, that the ground remains moist for any length of time. Then is the only chance for herbaceous vegetation whether endemic or introduced.

and under its influence the seeds germinate; hereupon the temperature suddenly falls, and the young and tender seedlings are exposed, at a critical period in their career, to entirely new and unfavourable conditions, and they perish accordingly. It is therefore no matter for wonder, and still less for drawing conclusions as to "aggressive power" and "superiority" of the northern species, if introductions from the northern hemisphere are enabled to exist and multiply in the southern, while an embargo is placed upon southern species in Northern and Central Europe. Moreover, that this is the real reason why southern species are not domiciled with us seems clear when it is remembered how, in northern countries where the conditions are approximately similar to those obtaining in the southern hemisphere, southern introductions are able to maintain themselves. One may cite, for example, the Western Mediterranean seaboard and the coast of Portugal, where a fair number of southern species—most of them, it is true, South African, from greater facility of intercourse—have succeeded in establishing themselves, and apparently at some expense to the indigenous flora.

There is one country north of the equator where Australian species readily become naturalised. Botanists who hold fast by the theory that the Australian flora is a mere geographical survival have been puzzled—as assuredly they ought to be puzzled—by the headway that species from Australia make when introduced into Southern India; nor does Mr. Wallace's solution of the problem, ingenious though it be, at all relieve matters. Mr. Wallace cheerily avers that this fact is quite in harmony with the presumed predominance of northern forms. "For," he says, "not only is the climate favourable, but the entire Indian peninsula has existed for untold ages as an island, and thus possesses the insular characteristics of a comparatively poor and less developed flora and fauna as compared with the truly continental Malayan and Himalayan regions. Thus Australian plants can compete with a fair chance of success."[1] But what evidence is there for Mr. Wallace's idea? We venture to maintain, on the contrary, that the Indian flora is, in all essentials, a continental one, and, moreover, the "untold ages" Mr. Wallace alludes to are scarcely in point, for what we want is evidence as to the continued insularity, in a botanical sense, of a region which, for many thousands of years at least, has ceased to be an island. But why travel so far in search of an explanation when one is ready to hand? Why not admit that Australian species flourish in the Neilgherries simply because the present climate of that district is suitable to them? And why not go a step further, and allow that if a land connection existed between Australia and South India, and the intervening country enjoyed a climate like that of Australia, a considerable number of

[1] "Island Life," p. 496, note. The fact there cited was communicated to Mr. Wallace by Sir Joseph Hooker.

Australian species, or of descendants from such, would to-day form part of the Indian flora? But if this be admitted, and it is only a logical deduction from the facts, the theory of the predominance of northern forms collapses, and the restricted area occupied by Australian species must no longer be viewed as depending upon some inherent inferiority to northern forms, but simply upon fortuitous geographical conditions.[1]

But we are told that the Australian flora stands less high in the scale and is less specialised than are the floras of northern climates, and if this be true, the point I am trying to argue must at once be given up. But is it true? In what respect, it may be asked, is the flora of Australia less highly specialised? Are not most of the great natural orders strong constituents of it? Trees, some of them of gigantic size, shrubs, undershrubs and herbs, parasites and saprophytes, climbing and carnivorous species, flowers adapted to profit by the visits of insects, and sometimes provided with a complex mechanism to ensure such profit, all these are met with in Australia. In addition, we have wonderful adaptations to a dry climate, and in this respect, taking into account the variety of ways in which the destructive effects of a scorching sun and parched soil are guarded against, the Australian flora is without a parallel the world over. And if these be not evidences of high specialisation, it is difficult to know where one must look for such. In one respect, and in one only, is any inferiority shown, namely, in the comparatively small number of seeds produced. But this does not apply to the herbs, and as for the woody species, it is absolutely essential that the ripening seeds be safeguarded against drought, and the laying on of thick tissues to this end may well be effected at some cost as regards fecundity.

But Mr. Wallace himself gives us an instance where land adjoining the, according to him, previously isolated home of the Australian flora has been stocked to a considerable extent with Australian forms. As I shall have something to say hereafter about this supposition, I will now merely assume its truth for argument's sake. Mr. Wallace,[2] then, supposes the greater part of Northern Australia, previously submerged beneath the ocean, to have become dry land in the middle or latter part of the tertiary period, and the area so exposed to have been colonised partly by Indo-Malayan forms from the north, partly by Australian forms from the south. Now, assuming with Mr. Wallace that the species with Indo-Malayan facies in Northern Australia were emigrants from the north, their considerable numbers prove that there could have been but slight, if any, embargo upon migration from the north when

[1] Since this passage was written, Mr. C. B. Clarke has informed me, upon his personal knowledge of the Neilgherries, that the success of Australian species there has been much exaggerated. In spite of this, I prefer to leave the paragraph as it stands, for it shows, at any rate, to what lengths an upholder of the "northern predominance" theory may be inclined to go when in search of an argument to meet alleged facts hostile to the theory.

[2] "Island Life," p. 493.

Northern Australia was stocked. Why, then, if Australian forms are less highly differentiated and less capable of adaptation than Indo-Malayan, do we find them holding their own to-day side by side with the more favoured northern migrants? Assuredly this is precisely what we ought not to expect if the theory of northern predominance be sound. We ought rather to expect that those migrants from the south which happened to penetrate into the newly raised area would have been rapidly overcome by their better adapted competitors; and the fact that they have not been so overcome should suffice to convince us that, supposing Mr. Wallace's view of the stocking of Northern Australia to be correct, Australian species can compete not unsuccessfully with Indo-Malayan ones in the struggle for existence on a fresh area. In short, what Mr. Wallace supposes to have actually happened in Northern Australia is exactly what I have just now surmised might have happened in India, but for the wide stretch of intervening sea which has prevented Australian forms from entering the Indian peninsula.

And when we come to consider the extinctions that have taken place in the Australian flora since earlier tertiary times, we find ourselves face to face with a number of facts which contradict *in toto* the doctrine of northern predominance. The only way of escaping from these facts is to deny the soundness of the conclusions upon which they are based, that is, to throw doubt upon the determinations of the palaeontologists. This is the position taken up by Professor Drude,[1] who not only denies that a flora in many respects more northern than the present flora formerly flourished in Australia, but also questions the former presence in the European flora of many species belonging to orders now characteristic of Australia. Professor Drude cites as an example the genus *Quercus*, which has a wide distribution in space, and contains species showing much adaptability to diverse conditions, facts rendering it difficult to understand how such a genus could disappear from any large area it formerly occupied. This instance, however, is not a very happy one, for *Quercus* is now known to flourish in New Guinea, and it may still be found living in Australia when the northern part of the island-continent has been more thoroughly examined. Moreover, we are only imperfectly informed as to why species become extinct. Why, for example, should so few Brachiopods now tenant our seas? Why is it that the great group of the Ammonitidae, so abundant in Mesozoic times, is represented to-day by but one solitary survivor, or, as some may say, by none? What reason can be given for the extinction of the numerous mammals characteristic of earlier tertiary times? The general principle underlying extinction is, of course, a mere commonplace to-day: it is the application of it to individual instances that is obscure; so much so indeed that, in spite of Mr. Darwin's injunction to a contrary view, I do hold, with all due

[1] "Handbuch der Pflanzengeographie," s. 450.

deference, that a fact such as the survival of *Lingula* through countless ages; while multitudes of closely related and equally effective forms have long been extinct, is not devoid of the element of mystery. Such a consideration as that adduced by Professor Drude seems wholly insufficient to outweigh the life-labours of men like Unger and Goeppert, Heer, Ettingshausen, and others. True, their determinations may sometimes be open to objection; but in such a case as this there seems no alternative but to accept, as correct in the main, the conclusions unanimously recorded by specialists in this branch of the science. When, therefore, one finds in the Australian tertiary flora such characteristically northern genera as *Myrica, Betula, Alnus, Quercus, Salix, Fagus, Laurus, Magnolia,* all of which, with the exception of *Fagus,* now scantily represented on the south-eastern highlands, and possibly of *Quercus* as mentioned above, have vanished like the fantasies of a dream, one cannot repress a feeling of wonder that such a phrase as "the Scandinavian privilege of ubiquity" should ever have been called into use. Most of the above genera, if present distribution is to be relied on, and present distribution is the main support of the northern predominance theory, have had their origin in the most extensive land area of the globe, where, according to Mr. Darwin, competition has been most severe and long-continued, and moreover they are still important elements in the northern flora. On the current hypothesis these favoured forms should have entirely or partially eliminated their competitors, instead of which they have themselves gone to the wall. But besides this we are not entitled to assume that Australia was inhabited in earlier tertiary times by no other "northern" genera than have already been found in tertiary deposits there. It is also inconceivable that herbaceous vegetation did not then exist side by side with the shrubs and trees whose harder parts have ensured their preservation in the fossil condition. But before we are in a position to state what this herbaceous vegetation really was, Australian tertiary deposits must be examined in the way in which Mr. Clement Reid is now examining our tertiary beds with such interesting results, for the ordinary organs of herbs are of too fragile and evanescent a nature to allow of their preservation, and recourse must be had to the evidence yielded by fruits, and especially by seeds, involving a tedious operation indeed, but one which must be undertaken before we can feel ourselves on safe ground. Meanwhile we cannot close our eyes to the possibility that a fair number of herbaceous species belonging to "northern" genera may have become extinct in Australia since the time when the "primitive tertiary flora" flourished there.

And while we recognise how favourable to the northern flora are the geographical and climatal conditions of Northern Europe at the present time, it should not be forgotten that such was not always the case. In Miocene times, for instance, when Greenland enjoyed a climate similar to that of Southern Europe to-day, where was the

"Scandinavian" flora? A considerable portion of it must have been in existence then, and it is difficult to conceive how the ancestors of so large and important an element in the earth's vegetation could have found sufficient room in the few extreme northern lands then suitable to them. But during Eocene and Miocene times a large part of the antarctic continent must have had a climate suitable to the support of "Scandinavian" forms; and if we can suppose, and there seems little difficulty in the supposition, warranted as it is by facts of distribution, that the antarctic continent was then continuous with South America, and had outlying lands permitting of interchange with South Africa and Australia, a portion, and no inconsiderable portion, of the flora now considered to be of northern origin may well have taken its rise in these southern lands. It was probably during the Pliocene period that the Scandinavian flora first became important in Northern Europe. Pliocene times must have been highly favourable to the diffusion of herbs which flourish best in colder temperate climates, for not only did cold conditions then prevail, but there were ready for colonisation large areas raised during the mountain-making Eocene and Miocene periods. It is conceivable, therefore, that much interchange between northern and southern lands may have taken place during this period.

But it may perhaps be that the Pliocene age is too recent for such a relation as has been sketched to have existed between the antarctic continent and lands lying to the north of it, though the recent discovery in South America of a carnivorous Marsupial allied to *Thylacinus* suggests that such a relation existed during later tertiary times. Yet the point to be remembered is that large areas in the south have enjoyed a climate eminently suitable to the evolution of forms best fitted to flourish in the colder temperate zones, and, moreover, that during long periods the larger extent of such areas has been in the south. The problem, too, how southern forms could have reached the north is no greater than the problem how northern forms could have penetrated into antarctic lands. All we know is that a genus could have had its origin in but one area, and that, as regards temperate forms, there is much generic resemblance between the northern flora and the southern; but there is no justification for the view that all the genera common to both had their origin in the north and none of them in the south.

It is also necessary to receive with grave doubt any conclusion relative to the inherent superiority of certain floras as a whole over others, and this although several species of supposed northern origin are capable of ready acclimatisation in foreign lands, and can sometimes flourish at the expense of endemic forms; for in every flora there are species more widely diffused and with greater powers of adaptation than others. Has anybody ever argued, from the rapid spread of *Anacharis alsinastrum* in our streams a few years back, from the way

in which, for instance, *Galinsoga parviflora*, and species of *Aster* are enabled to maintain themselves in Europe, any inherent superiority of the American flora over the European? Yet argument of this kind we find constantly applied to the flora of Australia. Nor is present distribution an infallible index to the place of origin of a genus or species. To take two instances showing the general trend of argument on this subject as bearing on the flora of Australia: *Helichysum* and *Helipterum*, although well represented in Australia, are found also in other countries; consequently, it is said, they are exotic genera which have at some time migrated into Australia. Why may not they, as well as other genera, be descendants from the constituents of the "primitive tertiary flora"? Professor Tate partially adopts this view, for he remarks, *àpropos* of certain genera found fossil in tertiary deposits, such as *Ficus, Loranthus, Pittosporum, Santalum,* and *Cassia*—that most of these genera, "when viewed by their present geographical distribution, are considered Oriental; but in regard to their distribution in time they belong to a cosmopolitan flora, which originated in late Cretaceous times in Europe, North America, and Australia; hence their modern representatives may actually be descendants of primitive Australian species, and not modified immigrant forms."[1] But though he makes this highly important admission, in practice he adopts the conventional view, for we find him distinguishing "immigrant" genera and species from "endemic" ones with confidence as serene as though he had himself been privileged to watch, through long ages, all the various steps in the stocking of Australia. Of course the view I am advocating cuts both ways. The Cambodian *Centrolepis*, for instance, may possibly be the sole Indo-Malayan survivor of a genus which had its origin in the Indo-Malayan region, and migrated thence into Australia. So too *Patersonia* may be of Indo-Malayan origin: even *Casuarina equisetifolia* may be, for all we know, the original species from which its Australian congeners have been derived. Not until all later secondary and tertiary deposits have been thoroughly ransacked, and their respective relations in time established beyond dispute, will it be possible to fix upon that part of the earth where a genus or a species first made its appearance. Until this is accomplished our conclusions can rest on nothing more satisfactory than inferences from present distribution, which, unless they be applied with the utmost caution, may lead us far from the truth.

The most recent and, as having been deduced with full knowledge of modern geological discoveries and after personal inspection of part of the country, the most authoritative conclusions relative to the origin of the Australian flora are those of Professor Tate.[2] The Darling

[1] "Botany of the Horn Expedition," p. 131.

[2] Professor Tate's three memoirs, *The Influence of Physiographic Changes in the Distribution of Life in Australia;* Australia's Association for the Advancement of Science (1887); Inaugural Address, in the Association's volume for 1893, and the "Botany of the Horn Expedition" (1896), are most interesting contributions to the subject under notice.

range, he tells us, which is of granite, is capped by conglomerates doubtfully referred by Mr. F. T. Gregory to the Devonian age, but, perhaps, as suggested by Mr. Etheridge, really Mesozoic. Since Upper Devonian times there have always been land surfaces, at any rate in Eastern Australia, where there was partial interruption to absolute continuity during deposition of the Carboniferous rocks. The country presented the aspect of a vast archipelago while the extensive marine cretaceous beds occupying the low-level tracts of the interior were being deposited ; and not until the close of the Cretaceous period was the continent formed. These marine beds—the so-called Rolling Downs formation, of Lower Cretaceous age—were laid down in a comparatively narrow sea connecting the Gulf of Carpentaria with the Great Australian Bight, and there is no evidence for the existence of interoceanic connection since that age, that is to say for the tertiary sea of Professor Duncan and Mr. Wallace. Following close upon the end of the Cretaceous epoch was another submergence during deposition of the older tertiary strata ; but this did not involve so large an area, as these marine tertiary beds are not found more than fifty miles inland except round the Great Australian Bight and in the Murray Desert. After this, by unequal movements of depression, Central Australia became a lacustrine area, for the low-level deposits of this region are of lacustrine origin as their remains prove. Lacustrine conditions continued into Pliocene times, unless the formation known as the desert sandstone, which is of Pliocene age, be eolian, as Mr. Tenison-Woods conceives. The extinct rivers, the circumscribed lacustrine basins marked by their coincident sand-beaches, and the remains of large herbivores prove the climate of Central Australia to have been, up till comparatively recent times, much moister than it is to-day. The subsequent history of the district has been one of gradually increasing desiccation.

(*To be continued.*)

FRESH FACTS.

Microscopic Vivisection. Eugène Penard. "Sur les mouvements autonomes des pseudopodes," *Arch. Sci. Phys. Nat.* vii. 1899, pp. 434-445. Mr. Penard has made numerous experiments with excised pseudopodia of *Difflugia lebes*, which go to show that detached (non-nucleated) fragments behave for a time as if they formed a complete organism. During their ephemeral life they exhibit movements; they are attracted by plasmas identical with their own, and repelled by those which are unlike.

A Wonderful House. H. Lohmann. "Das Gehäuse der Appendicularien nach seiner Bildungsweise, seinem Bau und seiner Function," *Zool. Anzeig.* xxii. 1899, pp. 206-214, 4 figs. Dr. Lohmann studied at Messina the history of the house of *Oikopleura*. The foundations are laid in 3 to 4 hours by the energetic secretory activity of special oikoplast cells which form the component membranes and fibrils. The house once begun is quickly finished, and has not been more than a few hours in use before another begins to be built. But what is its use? The answer to this is perhaps the chief interest of this paper, for Lohmann finds that it is justified in three ways. It forms an effective trap for food particles; it serves as a locomotor organ; and it protects the inmate, who can "blitzschnell" leave its encasement and escape with its life.

Notochordal Canal in Man. A. C. F. Eternod. "Il y a un canal notochordal dans l'embryon humain," *Anat. Anzeig.* xvi. 1899, pp. 131-143, 17 figs. The author has satisfied himself that there is in the very early human embryo a distinct trace of a notochordal or archenteric canal which does not differ in its essential features from that known in other mammals.

Hibernating Swallows Once More. Alan Owston. "Swallows in Mid-Winter," *Annot. Zool. Japon.* iii. 1899, p. 29. In a letter to our Japanese contemporary, Mr. Alan Owston of Yokohama notes that on the 16th of December 1896 he saw a number of swifts (*Cypselus pacificus*) flying about, and that on the 1st of January 1898 he observed a couple of swallows (*Hirundo rustica gutturalis*) catching flies on the beach. "Is it possible that some swifts and swallows remain here throughout the whole winter, and if so do they hybernate in caves like bats?"

When a Snail Leaves its Shell. R. Welch. "Helices abandoning their Shells," *Journ. of Conchology*, ix. July 1899, p. 217. We had thought that a snail would leave its shell when the Greek Kalends came round, or a canny Scot committed himself to a definite opinion on the weather, but we were wrong again. For there have been repeated stories of late in circulation about snails wandering about in indecent nudity. The *fama* arose in regard to *Limnaea peregra*, but it seems that the more sedate *Helix pisana* and *Helix lactea* have gone in for similar frolics. They were well fed, Mr. Welch assures us, and not handled in any way. This is a "curiosity" which some one will surely soon convert into an interesting fact by telling us the reason why. Is it an atavism before death—a return to ancestral nudity?

FACTS OF INHERITANCE. WILLIAM BATESON and Miss D. F. M. PERTZ. "Notes on the inheritance of variation in the corolla of *Veronica buxbaumii*," *Proc. Cambridge Phil. Soc.* x. 1899, pp. 78-92, 1 pl. Abnormal flowers are of common occurrence in this species, and certain symmetrical forms of variation are especially frequent. Flowers taken at random on heavy clay arable land near Cambridge showed about 6 per cent with 3 petals, and about 1 per cent with two petals, and so on. The experiments described in this paper were undertaken to test whether there is any difference between offspring raised from abnormal flowers, and the offspring of normal flowers borne by the same plant. The evidence, though scanty, goes on the whole to show that there is, at all events in the case investigated, no well-marked difference between the offspring of normal and abnormal flowers.

A PATHOLOGICAL PIGEON. MICHAEL F. GUYER. "Ovarian structure in an abnormal pigeon," *Science*, ix. 1899, pp. 876-877. In a bird which was a hybrid between a Vienna white (*Columba alba*) and a common ring-dove (*Turtur risorius*), the ovary showed a large number of double eggs, that is, two or more eggs within a common follicle. Most of the larger eggs showed vacuoles appearing in connection with the substance of the sphere or yolk-nucleus; the nuclei in many cases seemed degenerating; mitotic division of the nucleus was never observed; many of the eggs, especially the larger ones, were undergoing absorption by means of phagocytes which were the transformed follicle cells. The doubling of the eggs seemed to be due in most of the smaller ones to division of the primordial egg cell and in the larger ones to fusion of contiguous cells. It is not determined that such abnormalities are connected with hybridisation.

SEX IN BEETLES. GILBERT J. ARROW. "On sexual dimorphism in beetles of the family Rutelidae," *Trans. Entomol. Soc. London*, 1899, pp. 255-269. The recorded examples of sexual dimorphism among beetles, other than those which consist in differences of development of various parts, such as the legs, antennae, or mandibles, are at present very few; but this is partly due to the mistake of referring males and females to separate species. In the heterogeneous assemblage slumped in the genus *Anomala* there is colour dimorphism in species from all parts of the world. The distinction consists not in any fundamental difference, but in the degree of development of the colouring matter, the male (except in two exceptional Mexican species) exhibiting a greater exuberance than the female, or the superposition of a darker hue. In *Anomala imperialis*, discussed in this paper, there is another apparent exception, the colours of the two sexes appearing to be unrelated. But experiment shows that the metallic purple colour characteristic of the male of this species is transformed by exposure to sunlight into a green like that of the female, so that here also the male form is obtained by an addition to that characteristic of the female.

THE AGE OF THE MANX SLATES. H. BOLTON. "The Palaeontology of the Manx Slates of the Isle of Man," *Manchester Memoirs*, xliii. May 4, 1899, No. 1, pp. 15, 1 pl. In this paper (also issued as "Notes from the Manchester Museum, No. 5") are described specimens of *Dictyonema sociale* and *Dendrograptus flexuosus*, found by the writer in small splintery masses of these slates. These indicate that "the stratigraphical position of the slates will be found ultimately to be either amongst the uppermost beds of the Cambrian system, or in the Arenig Series." This conclusion does not conflict with the evidence of the worm castings referred to *Palaeochorda* and *Chondritis*, or the doubtful *Asaphus* also discovered by Mr. Bolton, or the yet more doubtful *Lingulella*, figured by E. W. Binney in 1877. The author is to be congratulated on the light, little though it be, that he has been able to throw on this particularly obscure problem.

Sexual Dimorphism in Jurassic Nautili. G. C. Crick. "Description of new or imperfectly known species of *Nautilus* from the Inferior Oolite, preserved in the British Museum (Natural History)," *Proc. Malacol. Soc.* iii. pp. 117-139, Dec. 1898. The observations of Willey on sexual dimorphism in the recent Nautilus have satisfactorily dispelled any doubts as to the existence of such a character, and divergences between individuals of any fossil species may therefore be interpreted as due to sex. Of the eleven species here described, seven appear to present both a broader form (male) and a narrower form (female) occurring at the same locality and horizon. In some specimens also it has been possible to trace very clearly the position of the anterior boundary of the muscular attachment. A specimen of *N. bradfordensis* shows the black layer as a band enveloping the whorl immediately in front of the aperture. A few non-adult specimens are described; and it is interesting to note that the British Museum specialist definitely accepts the approximation of the last two septa as a criterion of maturity.

A False Fossil. J. S. Diller. "Origin of Palaetrochis," *Amer. Journ. Science*, vii. 1899, pp. 337-342. In 1856 Professor Ebenezer Emmons described two species of Paleotrochis from the so-called Taconie rocks of Montgomery County, in North Carolina, and regarded them as siliceous corals, and as the oldest representatives of animal life upon the globe. But Hall, Marsh, J. A. Holmes, and others denied their organic nature, whilst C. H. White almost as strongly advocated it. Mr. Diller determines the Palaetrochis rock as an acid volcanic full of spherulites, and concludes "that Paleotrochis, where most perfectly developed and composed of granular quartz, is the result of deposition after the spherulitic growths about it and within it had developed, but whether this deposition followed soon after that of the spherulites in the course of solidification, or took place in hollow spherulites (lithophysae), or resulted perhaps long subsequently at the time of rock alterations, is not so clear." But this seems clear that the Paleotrochis is no reputable coral.

Diplospondyly. W. G. Ridewood. "Some observations on the caudal diplospondyly of sharks," *Journ. Linn. Soc.* (Zool.) xxvii. 1899, pp. 46-59. It is a well-known fact that in Selachian fishes the vertebrae of the tail are twice as numerous as the caudal segments as marked by the spinal nerves and the intermuscular septa. Dr. Ridewood reviews the facts and comes to the conclusion, "that the condition of diplospondyly in the tail of sharks is secondary, but of ancient date; and, further, that it is purely adaptive, being calculated to maintain a due proportion between length of centrum and width of body, without diminishing the length of the muscle-segments. In the region of the body from the cloaca to the caudal fin, the demand for increased flexibility is prepotent over the normal tendency of the chondrified chordal sheath to segment in such a way that the centra are as numerous as the myotomes."

Teratologia. Bertram C. A. Windle. "Ninth report on recent teratological literature," *Journ. Anat. Physiol.* xxxi. pp. 507-526. In this valuable record, for the continuation of which all biologists should be grateful, Prof. Windle gives a clear and terse summary of recent progress. He gives references to 83 papers, and arranges the results under the headings:—experimental, general, duplicity, head and neck, thorax, abdomen, genitalia, and extremities.

SOME NEW BOOKS.

THE SILURIAN ROCKS OF BRITAIN.

Memoirs of the Geological Survey of the United Kingdom: The Silurian Rocks of Britain. Vol. I. Scotland. By B. N. Peach, F.R.S., A.R.S.M., F.G.S., and John Horne, F.R.S.E., F.G.S., with Petrological Chapters and Notes by J. J. H. Teall, M.A., F.R.S., F.G.S. Royal 8vo, pp. xviii. + 749; xxvii. plates, 121 figures in the Text, and a coloured Map on the scale of ten miles to the inch. Published by order of the Lords Commissioners of H.M. Treasury, 1899. Price 15s.

For some reason that has not yet been discovered, the older rocks of Scotland appear to have been formed under somewhat different conditions from those which prevailed when rocks of the same age were in process of formation in other parts of the kingdom. Not only is this the case with regard to their original characters, but it is equally so with regard to their subsequent history. Nature's forces appear to have attacked the older rocks of Scotland more energetically than has been the case elsewhere; and, as a consequence, their present arrangement is much more difficult to make out than that of those, for example, which are in the Lake District. The Cambrian and Pre-Cambrian Rocks of Scotland have been deformed, and their order deranged, to an extent which is almost without a parallel outside of the Alps; and even those rocks which were formed between the close of the Cambrian period and the commencement of Devonian times have fared, in this respect, hardly any better than their predecessors. Hence the task of deciphering the geological history of the Ordovician and Silurian Rocks of Scotland has presented so many difficulties that it has repeatedly baffled the efforts of even the ablest geologists. It is quite true that each observer who has tried to work out the geological structure of these rocks has added something of value to the common stock of information; but it is now obvious to those who look back upon the methods of work adopted by these earlier geologists, that most of them had gone upon the wrong lines. As a consequence of this fundamental error, our knowledge of the succession of geographical events to which these rocks were due, proved to be almost as defective as was our knowledge of the sequence of biological events of which these rocks contain a record.

The reason why so many able men failed to read the history of these Scottish Ordovician and Silurian strata aright is sufficiently plain to us, now that our eyes are opened. It lay in the fact that, for some inexplicable reason, it has long been the fashion in Scotland to ignore the fact that geology is quite as much concerned with the past history of Life upon the Earth as it is with the physical history of the old sediments in which the vestiges of that life have been entombed. In the great majority of cases a student has been trained to regard the mineral constitution of some rock, let us say, for example, a dyke,

as a matter of vastly greater importance than the history of the fossils occurring in the strata which that dyke happens to cross. Whether the dyke consisted of basalt or of "melaphyre," or whether it should be called a dolerite or a "diabase," has in Scotland only too often been considered a question of far greater importance than whether the graptolites which occur in the strata traversed by that dyke indicate that the rocks are of Arenig age, or whether they date from Wenlock times, or, again, whether they represent any period of intermediate age. We cannot all be specialists, it is true; but, clearly, every modern geologist should be familiar with at least the zonal fossils of the rocks amongst which he is at work. One would also think that his work would prove of much greater interest to him if he knew something of the biological relationships of the organic remains with which he is likely to meet. As things stand at present, it may be confidently stated that, taking the whole of Scotland, the number of those who are really working at fossils of any kind may be counted on one hand—one of the authors of the present work being one of them. And even the number of those who are systematically making collections of fossils probably does not exceed a score. The case, of course, is very different south of the Border, where nearly every geologist takes a more or less keen interest in Palaeontology.

That these defects will soon be made good no one who carefully studies the most admirable historical introduction given in one of the earlier chapters of the book under notice can for a moment doubt. The whole of that history leads up to a triumphant vindication of the claims of Palaeontology to occupy a foremost place in the studies of all geological students in the future, not only on account of the light which that science throws upon the evolution of existing forms of life, but also on account of the invaluable aid it affords in unravelling the complicated structure of districts like that of Girvan, or of the Valentians or Southern Uplands of Scotland. Had it not been that Professor Lapworth brought to bear upon the rocks of these districts a combination of skill in field work with an extensive knowledge of Palaeontology, we should probably still have been no wiser regarding the true history of the rocks in question than we were thirty years ago.

On taking up the work whose contents have suggested these remarks, the reader will do well to give a full consideration to the section of the book referred to. He will find in it evidence of a strongly-marked desire on the part of the authors to deal in a generous spirit with the work of all previous observers, and he will further see how each man has added something of his own to our knowledge of these difficult rocks, and how that intellectual giant amongst geologists, Professor Lapworth, largely by working out the zonal distribution of the Graptolites, has enabled us, in the end, to gain a clear view of the true succession of the Scottish Ordovician and Silurian Rocks. By the light thus presented, Messrs. Peach and Horne, with Mr. Macconochie, have laboriously worked over the whole area where these rocks occur, and, bringing to bear upon them the results of wide experience, they have completed the survey of the whole area of which this book treats. It is from the vast mass of material collected in the course of this work that Mr. Horne has completed the present Memoir. No one who takes the trouble to read any section of it can fail to see that, in all respects, it forms a perfect model of what such a book should be. It may truly be said to present all that can be known at present regarding the geology of the group of rocks to which it specially refers, and Sir Archibald Geikie is to be congratulated on the production by his staff of a Survey Memoir in which the work of eminent specialists like Mr. Teall, Professor Lapworth, Dr. Traquair, as well as Professor Laurie and Mrs. Robert Gray, has been skilfully incorporated with the vast mass of information collected by the above-named members of the field staff of the Survey.

It may be well to mention here that the various geological maps, rock specimens, and most of the fossils, referred to in the Memoir, are exhibited in

the Gallery devoted to Scottish Mineralogy and Geology in the Edinburgh Museum of Science and Art.

It is no easy matter to give a summary of the contents of a book which contains in a highly-condensed (though perfectly lucid) form, so enormous an accumulation of facts. To the readers of *Natural Science* probably the chief interest of the work will centre upon the palaeontological portion, and upon such parts of the work as are more or less directly concerned with the Life of the Past; but we may, nevertheless, briefly notice its contents as a whole:—

The earlier chapters of the history bring before us records of a submarine volcanic episode, during the latter part of which the chief organic remains which were entombed in the sediments belonged to the Tetragraptidae, Phyllograptidae, and a few other Arenig forms of graptolites, together with one or two Phylloped Crustacea, and a few Inarticulata, representing the Brachiopoda. Next follows a record of much deeper water conditions, during which a large area of what is now Southern Scotland would appear to have lain at the bottom of an ocean more than 2500 fathoms in depth. It was at this time that the now well-known Arenig Radiolarian Chert was formed. (It may not be generally known that Mr. Peach was really the first to recognise the true nature of this deposit, and that named specimens of it were exhibited in the Gallery of Scottish Geology and Mineralogy in Edinburgh a year or more before any published description appeared.) Following this ancient oceanic ooze comes a record of frequent oscillations of level, and of a gradual elevation of at least the western part of the district to above the level of the waves. In the meantime the Arenig forms of graptolites had died out, new generations of Rhabdophora had gradually come into being, and the conditions favourable for the evolution of group after group of new species and genera appear to have continued, in certain areas, as around Moffat, for an interval of time of incalculable length. Then follows another and lengthy period, during which we have perfectly clear evidence, in other areas, of the gradual appearance and disappearance of whole families of Coelentera, Brachiopoda, Trilobita, and Arthropoda, as well as of other organisms; and evidently also (although the earlier chapters of this part of the history are yet wanting) of the gradual evolution of the ancestral forms of the Vertebrata. One of the most interesting features in the book is the record of the discovery of fish remains in the higher beds of the Silurian Rocks. These fossils have already enabled Dr. Traquair to throw a flood of light upon some points that had previously remained in obscurity; and there can be little doubt that we shall shortly learn more still, as the beds that yielded these organisms continue to be diligently searched. The closing episode of the Silurian Period in Scotland was one in which the marine conditions which had so long endured gradually came to an end. Continental conditions took the place of oceanic, terrestrial volcanoes arose upon what had formerly been the sea-bottom, and the Silurian sea finally gave place to the deserts within which the Old Red Sandstone was formed.

It is chiefly in connection with the eruptive and metamorphic rocks which date from this Devonian period, that Mr. Teall's numerous and valuable contributions have been given. Like the Stratigraphical and the Palaeontological parts of the book this Petrographical part cannot be summarised, for the simple reason that, from beginning to end, the work is already as closely condensed as it can possibly be.

Regarding the book as a whole one may confidently state that it is the finest geological monograph that has yet appeared, at home or abroad, and that it reflects the highest credit upon every one concerned in its production.

J. G. G.

THE PROPER STUDY OF MANKIND.

Man Past and Present. By A. H. Keane. Cambridge Geographical Series. Pp. xii. + 584, with 12 plates. Cambridge University Press: C. J. Clay & Sons. 1899. Price 12s.

Linguistic and literary attainments are as essential to the specialist in the field of Ethnology, as keen-edged tools are to the skilled artizan. A perusal of "Man Past and Present," by Prof. Keane, amply proves that, in addition to these accomplishments, the author is conversant with the vast amount of anthropological literature which has come into existence since the banner of Evolution was first raised by Darwin and Wallace some forty years ago. The volume now before us is the second which has appeared within the last few years from the pen of Mr. Keane on the same fascinating subject. The first, under the title of "Ethnology" (1895), was upon the whole well received by general anthropologists, although several critics pointed out its inadequacy to supply the recognised want of a compendious handbook to Ethnology in the English language. The subject-matter was treated in two divisions—(1) Fundamental ethnical problems, and (2) the primary ethnical groups—the first being unnecessarily long, and the second irritatingly short, and altogether unsatisfactory. The present volume furnishes, at least to some extent, the deficiencies of the former. But unfortunately in avoiding Scylla the author has fallen into Charybdis, by having to repeat in his new book much of what had already been said. In "Ethnology" the ethnical groups (less than half the volume) are discussed under *Homo Aethiopicus*, *H. Mongolicus*, *H. Americanus*, and *H. Caucasicus*. In "Man Past and Present" the subject is continued in several chapters on "Negroes," "Mongols," "American Aborigines," and "Caucasic Peoples." It is like an author who, having four tales to relate, and finding that he could not do so in one volume, publishes the first half of each tale in one book, and the concluding portions in a second book, both volumes being actually under different names. We greatly regret this disposition of the materials, as we are convinced that by a little re-arrangement of the anthropological problems, together with a curtailment of lengthy disquisitions on secondary details, so as to bring them more into harmony with the ethnological section, Mr. Keane had the opportunity of producing one book which would, undoubtedly, have been a great boon to students.[1] Moreover, both volumes are weakened by a division of the illustrations. We have, however, pleasure in quoting the following remarks from the preface which, while explanatory of the *raison d'être* of two separate books, gives an excellent résumé of the contents of the volume, as well as a specimen of the author's style:—

"In the preface to the 'Ethnology' a promise was held out that it might be followed by another dealing more systematically with the primary divisions of mankind. The present volume appears in part fulfilment of that promise. In the 'Ethnology' were discussed those more fundamental questions which concern the human family as a whole—its origin and evolution, its specific unity, antiquity, and primitive cultural stages, together with the probable cradle and area of dispersion of the four varietal divisions over the globe. Here these divisions are treated more in detail, with the primary view of establishing their independent specialisation in their several geographical zones, and at the same time elucidating the difficult questions associated with the origins and inter-relations of the chief sub-groups, and thus bridging over the breaks of continuity between 'Man Past and Present.'

"The work is consequently to a large extent occupied with that hazy period vaguely called pre-historic, when most of the now living peoples had already

[1] Such an ideal work already exists in the French language in "Les Races Humaines," by Dr. R. Verneau.

been fully constituted in their primeval homes, and had begun those later developments and migratory movements which followed at long intervals after the first peopling of the earth by pleistocene man. By such movements were brought about great changes, displacements, and dislocations, involving fresh ethnical groupings, with profound modifications, or even total effacements of racial or linguistic characters, and complete severance from the original seats of the parent stocks. In some cases the connecting ties are past recovery, so that the ethnical, like the geological, record must always remain to some extent a mutilated chapter in the history of the world and of humanity. But in our times many of the more serious gaps have been often most unexpectedly made good by the combined efforts of philologists, physical anthropologists, and especially archaeologists, who have come to the welcome aid of the palethnologist, hitherto groping almost helplessly in this dark field of human origins."

Mr. Keane is a "monogenist," and maintains that all the varieties of the human race can be traced back to one centre of evolution. The first splitting of the main stem was almost simultaneously into the three types—Negro, Mongol, and Caucasian—which still represent mankind on the globe. *Homo Americanus* is a great puzzle to ethnologists, more especially as the tendency of the most recent investigations is decidedly against the theory that palaeolithic man of quaternary times ever existed on the North American continent. By successive divergences from these three primary branches under the moulding influences of cross-breeding, and climatal, geographical, and other changes in the environment, Mr. Keane accounts for all the varieties of shadings which characterise and distinguish the present inhabitants of the globe. The "cradle-land," from which *Homo sapiens* first emerged and bade farewell to his congeners of the brute creation, was, according to the author, a lost continent, "Indo-African," now represented only by Madagascar and a few islands in the Indian Ocean. Of the three divisions of mankind still living, the Negroid ("Negrito") type is regarded as most nearly approaching the original form of tertiary man. On the *modus operandi* of this primary stage of humanity he quotes from Dr. Munro's writings on the influence which the erect posture played in the higher development of the brain, with regard to which he states (page 7):—"This greatly strengthens the view always advocated by me that man began to spread over the globe after he had acquired the erect posture, but while in other physical and in mental respects he still differed not greatly from his nearest akin."

The three chapters dealing with the Caucasic peoples will be found of greatest interest to general readers of anthropology. Here some of the more burning problems of the hour, bearing on early European civilisation, are intelligently discussed; nor does the author by any means submerge his own individuality in the various controversies which he summarises for his readers. He follows Prof. Sergi in assigning the Iberians, Ligurians, Pelasgians, etc., to an original home in North Africa. The "Mediterranean race," from whom a stream of "migration set steadily and uninterruptedly into Europe throughout both Stone Ages," was dolichocephalic, short in stature, and of a dark brown colour.

The task which Mr. Keane has set before himself in the compilation of this most readable book is one which few anthropologists would undertake, and which still fewer are competent to execute. He gathers his materials, apparently with great linguistic facilities, from far and wide—not always, however, from the original investigators, who are too often allowed to disappear, while the second-hand compilers are brought to the front. But, in extenuation, this much must be acknowledged, that his authorities are most faithfully given—and this is one of the most valuable features of the book. Scarcely a subject in the whole range of Anthropology and pre-historic Archaeology is omitted by this versatile author. Archaeologists, geologists, philologists, fólklorists, and even modern globe-trotters come on and go off the stage with startling suddenness.

Yet, amidst the diversified and world-wide dramas thus depicted in a long series of bygone civilisations, the author moves with much freedom and elasticity, bestowing here and there, as the case may be, a word of praise or dispraise. Altogether, Mr. Keane's book (of course including its predecessor as an integral part) is to be highly commended, not only on account of the general soundness of the opinions upheld, but also because of the interesting manner in which he has marshalled his facts. Nor will beginners in the study of Anthropology object to read the two volumes, notwithstanding a certain amount of repetition, for in both the author carries with him the attention of intelligent readers.

R. M.

THE ZOOLOGISTS IN CONGRESS.

Proceedings of the Fourth International Congress of Zoology. 8vo. Pp. xv. + 422, 15 pls. London, 1899.

This bulky volume forms no exception to the rule that the official "Proceedings" of Societies or Congresses are usually somewhat disappointing. It is true that the value of an international meeting of zoologists can hardly be estimated by that of brief abstracts of papers and speeches, but it is difficult to avoid a slight feeling of disappointment that the personal contact of so many specialists should produce apparently so little result, and that so many of the discussions should end in nothing.

On general subjects one of the most interesting papers is that by Prof. Mitskuri on zoology in Japan. In a brief historical sketch of the progress of natural science in that country, he shows that the common belief in its sudden rise within recent years is quite unfounded, and that the present condition of affairs is merely the natural outcome of generations of preparation. From the interesting account of scientific education at the present day in Japan we cull one little fact only. The biological students of Tokyo University are required to spend *at least* one season at the Marine Station in connection with the University, while those who take up zoology as a speciality spend much more time than this at the seaside. We recommend this regulation to the notice of some Western Universities.

Of the general discussions those on the position of sponges and on the origin of mammals are reported in some detail. As to the sponges there seems practical unanimity that they are not Coelenterates, but there is more doubt as to whether they are to be regarded as a separate phylum of the Metazoa, or as having originated from the choanoflagellate Protozoa independently of the other Metazoa. The position adopted depends upon the views held as to the meaning of the reversal of the germinal layers during metamorphosis, but the discussion of this point when pushed to extremes largely resolves itself into a juggling with words.

The discussion of the origin of mammals contains much that is interesting. While Professor Haeckel still adheres to the earlier position that the placentals are descended from a marsupial stock, most other zoologists seem to regard Hill's discovery of a deciduous allantoic placenta in *Perameles* as conclusive proof that placentals and marsupials have arisen from a common stock and form parallel phyla. As to the more remote ancestry there is much more doubt and great difference of opinion. Prof. Osborn believes that mammals arose from the theriodont division of the anomodont reptiles, and that they are diphyletic, the marsupio-placental stock arising at the time when the Theriodontia conserved a number of Amphibian characters. Prof. Seeley, on the other hand, believes that anomodonts are not the ancestors of mammals, but that both originated from a common unknown stock. On the other hand, Prof. Marsh rejected the suggestion of reptilian affinities altogether, and looked for the ancestors of mammals among early amphibians. All were agreed

in placing the point of origin far back, in Silurian or Devonian times, so that there is a certain fitness in the closing speech, that of Mr. Sedgwick, in which "pre-Cambrian times" are suggested as the period of origin, not of mammals only, but of all the "great classes of the animal kingdom." Mr. Sedgwick suggests that "the main part of the evolution of organisms must have taken place under totally different conditions to those now existing, and must remain for ever unknown to us." We duly altered our belief in Recapitulation to meet Mr. Sedgwick's criticisms, and have learnt to hold the cell-doctrine lightly at his bidding, but this new instance of "thätige Skepsis" makes so heavy a demand upon our credulity that we prefer to regard it as a delicate piece of sarcasm.

Among other interesting papers is one by Messrs. Mesnil and Caullery on polymorphism, and the occurrence of epitokous forms in the common littoral annelid *Dodecacaria concharum.* They find that the common form (Form A) is viviparous, and apparently reproduces parthenogenetically; males at least have not been found, and reproduction takes place at a time when the males of the other forms are not yet ripe. The second form (Form B) is rare, and occurs in both atokous and epitokous forms. The modifications of form displayed are in all respects similar to those displayed by the Nereids and Syllids. The epitokous forms leave their tubes and become free-swimming. Very rarely a third form was found (Form C), which likewise becomes epitokous, but the changes are less marked than in B. Of this form females only were found. The authors are uncertain whether these forms are to be regarded as allied species or as constituting a polymorphic species. The point of special interest is that the phenomenon of epitoky has not previously been described in sedentary Polychaetes. It seems probable that it occurs much more frequently among Polychaetes than is at present suspected.

The volume is furnished with a bulky appendix, a considerable portion of which is taken up by "Correspondence on the Nomenclature of Lepidoptera," being the classified answers to questions circulated among certain entomologists by Sir George Hampson. Whether this will advance the science of entomology or not, we cannot undertake to say, but it can be confidently recommended alike to the psychologist and the student of human nature. If, as we are led to believe, systematic or other work is almost impossible to the entomologists, on account of the difficulties of nomenclature, there seems no reason why they should not occupy their time instead in classifying the views of their fellow-workers on various subjects, but the result seems slightly ludicrous to the onlooker.

The appendix also contains in full Prof. Hubrecht's paper on the "Development of the Placenta in *Tarsius* and *Tupaia*, with Observations on its Importance as a Haemopoietic Organ," which is fully illustrated by plates. The volume contains abstracts of numerous other papers in addition to those mentioned, but most of these have been previously published elsewhere.

N.

INSECTS.

Insects (Part II). By David Sharp, M.A., M.B., F.R.S. Being Vol. VI. of the Cambridge Natural History. Edited by S. F. Harmer and A. E. Shipley. Pp. xii. + 626 with 293 figures. London: Macmillan, 1899. Price, 17s. net.

A hearty welcome will be given by all students of insects to this concluding portion of Dr. Sharp's monumental work, the commencement of which appeared four years ago in the fifth volume of the "Cambridge Natural History." The volume now before us deals with the higher Hymenoptera, the Coleoptera, the Lepidoptera, the Diptera, the Thysanoptera, and the Hemiptera. It must be admitted that this arrangement of the orders of insects is unsatisfactory; the Lepidoptera, for example, are removed far from their allies the Trichoptera

(included among the Neuroptera in Part I.), and placed next to the Beetles, with which they have no near relationship.

The treatment of the various groups is, however, admirable. No fewer than 180 pages are devoted to the Bees, Wasps, and Ants, and the external form and habits of these most interesting of insects are fully described after the observations of Janet, Verhoeff, Marchal, Wasmann, and other recent naturalists. Internal structure should perhaps have received more attention; some details of the digestive and reproductive systems of the honey-bee might fairly have been expected. Dr. Sharp writes on the economy of the social insects with charming enthusiasm, freshness, and human interest. After recording Hoffer's confirmation of Godart's statement—made 200 years ago—"that a 'trumpeter-bee' is kept in some nests to rouse the denizens to work in the morning," the suggestion is hazarded that the hour when the trumpeting occurs (3 or 4 A.M.), caused the observation to remain discredited for two centuries! The section on ants and their ways is particularly good.

Most of Dr. Sharp's own entomological work has been done on the Coleoptera, and his account of this order will therefore be scanned with special interest. Undoubtedly some grouping of the numerous families of beetles into large divisions is very convenient and desirable. Our author adopts the well-known Lamellicornia (placed at the head of the order), Adephaga, Heteromera, Phytophaga, and Rhynchophora, while the many families which will not fit into any of these—the Clavicornia and Serricornia of former writers—are relegated to a group appropriately called the Polymorpha. The account of each family is illustrated by a figure of a typical species with its larva; an original figure of the remarkable stridulating-organ of a Passalid grub (p. 192) is worthy of special mention. The enigmatic Strepsiptera are doubtfully regarded as an aberrant group of Coleoptera.

The section on the Lepidoptera is full, more attention than usual being devoted to internal structure. In the account of the wing-nervuration it is a pity that the American nomenclature—familiar to readers of *Natural Science* through the papers of Mr. A. R. Grote—is not mentioned. In classification, Sir G. Hampson is followed, his key to the families from the "Moths of India" being reproduced in full. Dr. Sharp's views on protective coloration and mimicry are far from "orthodox." It is doubtless well that the Batesian and Müllerian theories should not be dogmatically preached as they have been by many writers. At the same time, Dr. Sharp is hardly as fair as usual when he writes, "We think it is clear that the explanation from our point of view is of but little importance," and when he refers to Prof. Poulton's "Colours of Animals" as "the case as stated by an advocate." Dr. Dixey's recent suggestive work in support of the positions attacked is not mentioned.

That most difficult order of insects, the Diptera, is next dealt with, and the account of the outer form, classification, and larvae of flies is admirably clear and well balanced, though the internal organs and the formation of the parts of the imago in the grub and pupa might well have received more attention. The Fleas are treated as a sub-order of Diptera. There is a good account of the small but interesting group Thysanoptera, which is rightly regarded by Dr. Sharp as forming a distinct order. In the reference to Uzel's recent beautiful monograph on these insects, it is implied that the work is entirely in Bohemian, whereas it contains a rather full German summary.

The concluding chapter, devoted to the Hemiptera, is admirable both in its morphological and systematic portions. The Lice (Anoplura) are doubtfully treated as a sub-order. The volume is beautifully illustrated, and the footnote references to literature are full and instructive. Indeed, little complaint can be made except to "ask for more." Could not the author have added a chapter giving us his views on insects as a whole, the relationships between their orders, the probable course of their evolution? Only the faintest echoes are to be found in this book of the bold and suggestive paper on insect classification read

by Dr. Sharp last year at Cambridge before the International Zoological Congress. Here he restricts himself to a record of the facts of insect life and structure, and perhaps by the absence of any trace of a phylogenetic tree he silently rebukes the rashness of younger men. GEO. H. CARPENTER.

DR. WILLEY'S RESULTS.

Zoological Results based on Material from New Britain, New Guinea, Loyalty Islands, and elsewhere. Collected during the years 1895, 1896, and 1897. By ARTHUR WILLEY, D.Sc. (Lond.), Hon. M.A. (Cantab.) Part III. pp. 207-356, pls. xxiv.-xxxiii. Cambridge University Press, 1899. Price 12s. 6d.

Part III. of Dr. Willey's "Zoological Results" contains articles by Dr. Gadow, Mr. Shipley, and the author. Dr. Gadow gives an interesting account of the variations to be found in the carapace of young chelonians. We must assume that the course of evolution in the chelonian branch of reptiles has been in the direction of a steady reduction in the number of scutes covering the carapace, in accordance with a "widespread evolutionary law" of the "specialised few" replacing the "generalised many."

The turtlets show a greater percentage of abnormalities in the carapace than the older individuals. "Our Turtlets start with many, with at least 24 dorsal scutes (leaving out the marginals), and then reduce them to 16. In other genera the reduction has advanced to 14, to 13, and individually to 12. This means onward development. The ideal, the goal for the young *Caretta*, is the possession of a 16-scuted shell. Those which start with 24 perhaps never reach the ideal, but this failure does not seem to hurt them, natural selection remains indifferent. Others start with 22, 21, 20, 19, or 18 scutes, and the latter individuals are rather common in the newly-hatched stage, and all of these seem to reach the goal. . . . These variations from the normal type all lie in the direct line of descent, and the more serious the variation the farther back it points. Moreover, the changes necessary to turn any given variation into another one less abnormal, until ultimately the normal condition is reached, are not erratic, but stand in strict correlation with each other, and proceed strictly on definite lines. I therefore call this kind of atavistic variation *orthogenetic.*" This orthogenetic variation in young chelonians appears to be a very striking example of Van Baer's law in its modern application.

Dr. Willey follows with a valuable contribution to our knowledge of the Enteropneusta. Firstly, he gives a synopsis of the groups under the three families of Ptychoderidae, Spengelidae, and Balanoglossidae, followed by a detailed description of *Ptychodera flava*, *P. carnosa* n. sp., *P. ruficollis* n. sp., with *Spengelia porosa*, Willey, and *S. alba* n.sp., with notes upon the West Indian species *Pt. biminiensis* n.sp., and *Pt. jamaicensis* n. sp.

There are many interesting points upon which one could dwell in these descriptions, but space will not permit. *Spengelia* appears to offer some remarkable features, including the so-called vermiform process of the stomochord (the latter is a useful name suggested by the author for the "notochord" of the Enteropneusta), and the presence of truncal canals. Dr. Willey finishes his paper by a discussion of the "Morphology of the Enteropneusta." He propounds a theory of the origin of gill-slits, based principally on their relationship to the gonads in this group.

Gill-slits primarily arose as inter-zonal depressions between the zonary, metamerically repeated gonads, functioning for the oxygenation of the gonads. Later they acquired openings into the pharyngeal wall, and were used for the respiration of the individual.

Further, he comes to important conclusions with regard to the stomochord of Enteropneusta and related organs in *Cephalodiscus* and Actinotrocha, which

cannot be dealt with here, especially as they are more fully stated elsewhere. He restates his former well-known conclusion of the homology of the vertebrate thymus with the branchial tongue-bars of Enteropneusta, and further finds the homologue of the endostyle in the parabranchial ridges, paired ciliated tracts which pass forwards to unite with the epibranchial band. This suggestion may be further compared with Garstang's comparison of the echinoderm ad-oral band with the endostyle.

Enough has here been said to show the value of Dr. Willey's contribution.

The third memoir is by Mr. Shipley, who takes the occasion to give a systematic revision of the groups of Echiurids. *Bonellia viridis* and four species of *Thalassema* are comprised in Dr. Willey's collection. The author gives a useful summary of the most valuable specific characters, of which the number of nephridia and the enumeration of muscle bundles appear the most important. The five genera, *Bonellia*, *Echiurus*, *Hamingia*, *Saccosoma*, and *Thalassema*, are dealt with.

From these brief remarks it will be noted that Part III. of the "Zoological Results" is full of interest alike to the morphologist and the systematist, and the author is to be congratulated upon his own labours and upon the able assistance which he has obtained. A. T. M.

REASONING MADE SIMPLE.

The Psychology of Reasoning, based on Experimental Researches in Hypnotism. By Dr. ALFRED BINET. Translated by A. G. WHYTE, B.Sc. 8vo, pp. 191. Chicago: The Open Court Publishing Company, 1899. Price 3s. 6d.

Dr. Alfred Binet's name is well known in association with that of Dr. Charles Féré (placed on the dedication page of this little book), to all who are interested in the phenomena of hypnotism. He here makes these phenomena throw such light as they can on the psychology of reasoning. His treatment has the advantage of perfect lucidity and of a simplicity which is, we venture to think, delusively alluring.

Reasoning is not regarded by Dr. Binet as a specialisation of conscious activity, and a differentiation only reached at a late stage of mental evolution, but rather as the general form of all psychical life. "To sum up," we are told, "all forms of mental activity are reducible to a single one—reasoning." "Three images which succeed each other, the first evoking the second by resemblance, and the second suggesting the third by contiguity—that is reasoning. Submit any reasoning to analysis, and you will find nothing else at the bottom of the crucible. But it would be an error to believe that this process belongs specially to reasoning. Far from it. We meet with it in all intellectual operations; it is the single theme upon which nature has embroidered the infinite variations of our thought." When a three-day-old chick avoids a cinnabar caterpillar as the result of previous experience of like objects, we have the three successive images; this caterpillar evoking images of certain others by resemblance, and these others suggesting the nastiness which was unpleasantly contiguous. Changing for convenience the order of formulation, and leaving out one little word, Dr. Binet gives for comparison—

> This is a crystal;
> All crystals have planes of cleavage;
> This has a plane of cleavage.

Here, he says in effect, this crystal is on all fours with this caterpillar; other crystals suggested by resemblance take the place of other caterpillars similarly suggested; while experience suggests cleavage in the one case just as it suggested nastiness in the other. But where does the *therefore* come in? In the

present state of psychological nomenclature it seems open to an author to define any term in accordance with his special predilections. We think, however, that the majority of reasoning men believe that the process demands a due comprehension of that subtle relationship among thoughts which we symbolise by ∴ or ∵ But this is perhaps because it is consonant with our own special predilections.

C. Ll. M.

A WELCOME WORK.

The Origin of the British Flora. By CLEMENT REID, F.R.S. 8vo, pp. vi. + 191. London: Dulau and Co., 1899.

Few works on the British flora possess greater interest or importance than this, which deals with the evidence gained during recent years from investigations into the vegetable remains of the later Tertiary and the Post-Tertiary deposits in Britain. These investigations rest mainly on the work of Mr. Reid himself, ably supplemented by Mr. James Bennie and other careful observers. Their results have been published through varied channels; and Mr. Reid has laid all interested in the flora of Britain under an obligation by bringing these results, and a good deal of other information, within easy reach. The author is peculiarly well fitted to perform such a work. Long-continued personal researches in Britain have been supplemented by wide acquaintance with the labours of others, both in Britain and throughout the north of Europe. He has produced a book that will do much to stimulate others to extend the work and to fill the gaps in the record in so far as that can be done. One part Mr. Reid might have extended with advantage to the recruits that the book is likely to enlist. The hints that he has given as to the most productive localities, and the methods of preparation of plant remains from the Tertiary and the Post-Tertiary deposits in Britain, make one feel how helpful a fuller treatment of both topics would have been. His remarks about the difficulty of obtaining fruits and seeds of existing plants with which to compare the fossils, emphasise strongly how imperfect herbaria are, as a rule, in the provision of complete examples of these parts.

The introductory chapters deal with the leading peculiarities and divisions of the existing British flora, the means of dispersal of the seeds met with among its members, and their consequent fitness for ready distribution; the changes in the form of the islands and their relation to the continent of Europe in former periods, and the evidences of changes of climate and their influence on the flora. A careful study of these chapters will aid much in arriving at clear views of the true nature of the problems involved in explaining "the origin of the British flora," and in accounting for its more marked peculiarities when compared with the floras of the adjoining countries.

Next follows an enumeration of the various localities in Britain (arranged alphabetically) from which these fossils have been recorded, with a notice of the probable age of each deposit, and a list of the species identified from it. Some continental localities are similarly treated. Then comes a list, in systematic order, of all existing British plants that have been identified as fossils, with a list under each of the localities in Britain in which it has been found fossil, or on the European Continent, if not yet found fossil in Britain; and the age of each plant as a fossil is given. The chief facts under this are briefly summed up in a "Table showing the range in time of the British Flora."

It is no mere form of words to say that the book is indispensable to all who wish to gain a clear conception of the nature of the British flora. This is evident from a single perusal of its pages; but its full value will be realised only after frequent and continued reference. Only six species, no longer found in a wild state in Britain, have as yet been identified with certainty as living in our islands in the later Tertiary or Post-Tertiary times. These are :—*Acer monspessulanum*, *Trapa natans*, *Salix polaris*, *Picea excelsa*, *Naias graminea*, *N. minor*. A

number of others are indicated by seeds or other remains that have not yet been determined, and there is evidently much work to be done in the field of study so well opened by Mr. Reid. J. W. H. TRAIL.

MICROSCOPY FOR BEGINNERS.

Chats about the Microscope. By HENRY C. SHELLEY. 8vo, pp. 101 (8 blank). The Scientific Press Ltd., London, 1899. Price 2s.

This is a nicely written and nicely printed little book, beginning with a brief account of the compound microscope, methods of mounting, etc., and going on to descriptions of various objects living, and otherwise suitable for examination. The descriptions are rather flowery than detailed; the lines are "heavily" leaded (*Anglice*, wide-spaced) to correspond with the extreme meagreness of the text. It belongs to a type nearly extinct; and, on the whole, we think it would be nearly as welcome a gift-book to a lad fond of natural history as Wood's "Common Objects of the Microscope," and more up to date. Most of the 30 figures are at least fair, but the plate of the hyaline *Stephanoceros* is nearly as grimy as one of Phil May's "Three Black Pearls," and the lovely *Micrasterias Crux-melitensis* is vilely caricatured. Still we think that it may have a fair sale through the opticians. M.

A PROFESSOR OF PHYSICS DEALS WITH ORGANIC EVOLUTION.

Die Enstehung des Lebens aus mechanischen Grundlagen entwickelt. By Dr. LUDWIG ZEHNDER, A. O. Professor of Physics in the University of Freiburg i. B. Erster Teil. Moneren. Zellen. Protisten. 8vo, pp. 256, with 123 figs. Freiburg i. B.: J. C. B. Mohr (Paul Siebeck), 1899. Price 6 marks.

The author has previously endeavoured in his "Mechanik des Weltalls" to refer all known physical and chemical forces to gravitation; and he here attacks the problem of life. From atoms he leads the reader gently to molecules, and from molecules to "Fistellen" (molecules aggregated in hollow cylinders), and before we quite know where we are we have reached the Protists. On the ascending path, the gradient of which has been skilfully made easy, our confidence is increased by two fundamental biological principles: the first, that substance endeavours to multiply; the second, that substance endeavours to adapt itself to the conditions of existence. It need hardly be said that the molecules and fistellae multiply in nutritive conditions, and have their struggle for existence like full-fledged organisms. A full discussion of the soul is reserved for the third part of the book. Perhaps by that time the learned author may have realised that the organism is not so simple as his theory suggests. In particular, we should desire more detail in regard to the origin of its power of adapting itself. X.

CHILD-STUDY.

Anthropological Investigations on One Thousand White and Coloured Children of both Sexes, the Inmates of the New York Juvenile Asylum. By Dr. ALES HRDLICKA. 8vo, 86 pp. New York, 1899.

The principal aim of these investigations is to learn as much as possible about the physical state of children who are being admitted to and kept in juvenile asylums. In the second place, this study is a part of the general anthropological work of the author, which is expected to result in an addition to our knowledge of the normal child, and of several classes of children who are, morally or other-

wise, abnormal. Cases where the parents were known have also furnished some data in regard to inheritance. The work has been carefully done, and the author's scientific temper is indicated by his refraining at present from any generalisations. We would echo his recommendation that the State Boards, and here as well as in America, should give their official sanction and support to such studies (without which our ameliorative devices will linger long on an empirical level), and should extend them gradually to correctional and other institutions, provided, of course, that the services of expert and unprejudiced investigators can be secured.

A PRACTICAL COURSE ON CYTOLOGY.

Praxis und Theorie der Zellen- und Befruchtungslehre. By Dr. VALENTIN HÄCKER, A. O. Professor in Freiburg i. Br. 8vo, pp. viii. + 260, with 137 figures. Jena: Gustav Fischer, 1899. Price 7 marks.

This book had its origin in the practical course of studies on the cell and fertilisation given in the Zoological Institute at Freiburg i. Br. Experience was thus gained in choosing the best material to illustrate particular points, and Dr. Häcker has made this available to other workers. The result is a practical handbook of great utility. It consists of lessons for sixteen days, and deals with forty objects, such as staminal hairs of *Tradescantia*, epidermis of salamander larva, *Amoeba* and *Pelomyxa*, *Stylonichia mytilus*, living nuclei from the bladder wall of the salamander, ovarian ova of newts, spermatozoa of the salmon and trout, leaf-epidermis of *Leucojum*, *Stentor coeruleus*, root-hairs, ovarian tubes of insects, corneal epidermis, testes of salamander, ova of *Ascaris*, *Thysanozoon*, *Canthocamptus*, *Anodonta*, *Myzostoma*, *Tegenaria*, *Echinus*, etc., hybrid larvae of sea-urchins, antherozoids of ferns, and so on. In each case the methods to be followed are clearly indicated. The lessons are intended to illustrate the structure of the cell, cell-division, oogenesis, spermatogenesis, reducing divisions and maturation, fertilisation, etc., and short discussions are interspersed dealing with the established facts and the current theories. Brief historical sketches of the progress of research are also given, and carefully selected references to literature. A brief general chapter on the cell concludes the volume. Opinions may differ as to the choice of objects, but all will probably agree that it was a happy thought on Dr. Häcker's part to place the results of his experience at the disposal of workers in other schools.

J. A. T.

THE FRIEND OF THE FISHERMAN?

The Lancashire Sea Fisheries: A Lecture delivered in the Chadwick Museum, Bolton. By C. L. JACKSON, M.Inst.C.E., etc., Presid. of Bolton Microsc. Soc. Pp. vii. + 85. Manchester: Abel Heywood and Son. London: Simpkin, Marshall and Co., 1899. Price 2s.

This was probably an amusing lecture to listen to, and interesting because of the personal reminiscences; but, unfortunately, the author has been induced by friends (so he tells us) to rush into print, and the little book, we fear, will serve no useful purpose and may be mischievous. Twenty to thirty years ago Mr. Jackson was evidently active as a fisherman and observer. He quotes from Buckland and Walpole, "Land and Water," and Reports of Fisheries Commissions of that date; and for him these statements are evidently conclusive, and the investigation of the sea which has been carried on since by nearly every civilised country either does not exist, or is only a fit subject for scoff and sneer.

The book is a venomous attack upon the Lancashire Sea Fisheries Committee, their methods and their administration, and is evidently written from

the point of view of one section of the fishing community—the shrimpers. The "Friend of the Fisherman" is much in evidence, and nothing is too bad for those who propose fishery regulation. Mr. Dawson and Dr. Herdman come in for a large share of the abuse.

The bane of so much "popular" fisheries literature at the present day (perhaps it was always so) is that the writers seem to think solely of what would be best for this or that set of men with whom they happen to have sympathy, instead of considering what is required in the interests of the public as a whole, not this year nor next, but for years to come. Y.

PETROLOGY FROM COOLGARDIE.

The Geology of the Coolgardie Goldfield. By TORRINGTON BLATCHFORD, B.A., F.G.S. Geological Survey of Western Australia, Bulletin No. 3. Perth, 1899. Pp. 98 and 2 plates.

This publication opens with a short account of the boundaries and history of the Coolgardie Goldfield, together with a statement of the opinions entertained by previous observers on the geology of the district. Then follow the author's personal observations. He cites Mr. T. A. Rickard's description of the deposits, in which the auriferous cement, having an average thickness of 2½ feet, is stated to rest upon a surface of decomposed granite. A capping of kaolin and sand-rock, the latter with seams of pipe-clay, rests upon the cement, this capping barely exceeding the thickness of the latter. The cement is less coherent than the "Banket" of South Africa. The observations of Mr. Göczel on these deposits are also quoted. The Kanowna Lead is described and its output given, the total yield of gold being estimated at 191,478 oz. 10 dwt. 22 gr. The ironstone gravel beds are next described, and then follow very admirable accounts of the granite, amphibolites, diorites, andesites, and schists of the district. The author is of opinion that the schists, which are hornblendic, or occasionally talcose, result from the surface weathering of amphibolites, and he adds: "As regards the amphibolites, there is little doubt in my opinion that they are so closely associated with the diorites as to be inseparable from them." The question of water-supply with details of borings is next dealt with, followed by important but concise descriptions of reefs. A couple of pages are devoted to an account of minerals found associated with the ore-bodies. Pages 51 to 78 give descriptions of the mines of the district. The remainder of the work is occupied by statistics, a diagrammatic representation showing the annual output of gold, and a coloured geological map of Coolgardie. Altogether this little publication is an admirable piece of work, one of which any survey might be justly proud, for besides being a treatise of great utility to a mining population, it is also a valuable contribution to petrology.

F. R.

THE LINNAEAN NAMES.

An Index to the Generic and Trivial Names of Animals described by Linnaeus in the 10th and 12th editions of his "Systema Naturae." By CHARLES DAVIES SHERBORN. Manchester Museum, Publication 25. 8vo, pp. viii. + 108. London: Dulau and Co. Manchester: J. E. Cornish, 1899. Price 3s. 6d.

It should be known to zoologists that the author of this Index has for some years been engaged in the compilation of an "Index Animalium." Pecuniary aid has been received from the British Association and from the Zoological Society of London, and we understand that nearly all zoological writings from 1758 to 1800 inclusive have been worked through, and that the names contained therein have been entered in duplicate on a slip-catalogue. It is hoped

that the question of printing and publishing this portion of the accumulated material will soon be ripe for discussion. Meanwhile the book before us, published by the enterprise and liberality of the Manchester Museum, serves as a *ballon d'essai*. It is in itself a work of much utility, and it shows the method that will be followed in the larger "Index Animalium." From that, however, the present index differs in the omission, as unnecessary, of the author's name (*e.g.* Linnaeus, "Syst. Nat.") after each item, as well as of any indication to what class of the animal kingdom each genus belongs.

Such a work scarcely lends itself to criticism. The text appears to us both clear and accurate. Mr. Sherborn has indexed the sponges, which are omitted from the German Zoological Society's reprint of the tenth edition. He has included the numbers which indicate the position of each species in its genus, a matter of some importance. In an Introduction he gives an annotated list of the editions of the "Systema Naturae," and points out the changes involved by accepting the tenth instead of the twelfth edition as the *ab urbe condita* of systematic zoology. Among these appears the name of the Dodo, henceforward to be known, not as *Didus ineptus*, but as—well, buy the book and find out!

We have received a descriptive Catalogue of the Tunicata in the Australian Museum, Sydney, N.S.W., by Prof. W. A. Herdman (8vo, xviii. and 139 pp., with 45 plates; Liverpool, 1899). It is what it professes to be, a descriptive catalogue, and not a monograph, but its usefulness is increased by an introductory account of the structure and life-history of a typical Ascidian, and by a list as complete as possible of the Tunicate fauna of Australian seas. The Trustees of the Museum were fortunate in securing the services of Prof. Herdman, who is one of the highest authorities on Tunicata, and the catalogue will be welcomed by zoologists at home as well as in Australia. The liberal allowance of plates adds greatly to the value of the work.

In *Science* for June 30 there is an interesting short article by Mr. Sylvester D. Judd, on birds as weed destroyers. "The goldfinches and native sparrows are more beneficial to agriculture than a number of other species, such as the English sparrow and blackbirds, which at times injure grain and fruit, but there are some fifty species of birds engaged in the work of weed-seed destruction, and the number of species of weeds which they tend to eradicate amounts to more than three score."

In the scientific section of the current number of *The Literary Digest*, which is conspicuously up-to-date, there are translations of papers on the alleged germ of cancer (Bra's organism); on how to make coloured people white (E. Gautier) by "depigmentising" them electrically—a paper which shows that the Ethiopian may at considerable expense and with no obvious utility change his skin; on the age of the Niagara Falls (Prof. G. F. Wright); on experiments as to the sensitiveness of school children, by that arduous worker Dr. Arthur Macdonald; and more besides.

In the number of the *Scientific American*, dated July 8, Dr. E. Murray-Aaron tells of the habits of the "honey-birds" which guide explorers to stores of honey, but with their own gratification for their "end and aim." It is also noted in the same number that some of the insects which pollinate the Smyrna fig have been made to establish themselves in California. The flavour of the "fruit" is said to depend upon the number of ripened seeds.

In *Science* for July 7 there is an excellent lecture by Prof. Charles Sedgwick Minot on "Knowledge and Practice," one of the central sentences being:—"Our greatest discovery in scientific teaching is the discovery of the value of the laboratory and its immeasurable superiority to the book in itself." Other points are the insistence on biology as an essential introduction to the study of modern

medicine, and the inculcation of the value of the comparative method, not in anatomy alone, but in physiology, pathology, embryology, and further.

Nature for July 6 has an interesting review of the latest work on Mammalian distribution :—"The Geography of Mammals," by W. L. and P. L. Sclater,—a work which should also have been sent to *Nat. Sci.* The review is of particular interest because of the antithesis, half expressed, and half repressed, between the reviewer's conclusions and those of the authors, an antithesis which forcibly suggests the rapid progress in this department of zoology.

Mr. L. L. Otler, a vice-president of the Selborne Society, proposes to have published "The Naturalist's Calendar or Diary," kept by Gilbert White of Selborne from January 1768 to June 1793 ; and subscriptions to this interesting work may be addressed to A. J. Western, Secretary of the Selborne Society, 20 Hanover Square, W. The price to subscribers is 30s. a copy, to others £2 : 2s. net.

The Geological Survey of Belgium is about to publish a "universal repertory of geological work," entitled "Bibliographia Geologica," edited by Michel Mourlon, Directeur du Service géologique de Belgique, with the collaboration of G. Simoens, D.Sc.

In *Nature Notes* for June is an article reprinted from the *Standard* newspaper entitled the "Vanishing African Fauna," which, however, contains little, if anything, that is not already recorded in Mr. Bryden's "Nature and Sport in South Africa," on which work it is apparently, indeed, mainly based.

Of more interest is a note in the same serial by Mr. R. Morley, calling attention to the very serious diminution in the numbers of a West African Guercza Monkey (*Colobus vellerosus*), on account of the persecution to which it is subjected for the sake of its beautiful and valuable skin. The Government of the Gold Coast (which is the one concerned) should intervene with a strong hand, and at once prohibit such destruction.

OBITUARIES.

CARL CLAUS.

Born at Kassel in Hessen, January 2, 1835; Died, January 18, 1899.

Prof. Karl Grobben briefly reviews[1] the life and work of the late Prof. Claus of Vienna, and gives a full list of his memoirs. The majority dealt with the Coelentera and the Crustacea, and a few with the more general problems of Biology. Of his writings that which was most widely read was the work which underwent many changes of form and title since its first (1868) publication as "Grundzüge der Zoologie," and its final (1883-1897) issue as "Lehrbuch der Zoologie." The Lehrbuch was, as Prof. Grobben informs us, Claus's "Lieblingswerk," and enjoyed an extraordinary and widespread popularity. As a teacher Claus emphasized the importance of adequate practical work, and as director of the Zoological Station at Trieste was enabled to supply his students with living material. The result was seen not only in the founding of the journal in which the present memoir appears, but also in the numerous students trained under him who now occupy professorial chairs in Austria and Germany. The personal character of Prof. Claus is summed up in the two phrases :—he was a "hervorragender Forscher" and a "lebhafter Kämpfer."

The following deaths are announced :—On June 24, at the age of 55, Charles William Baillie, marine superintendent of the Meteorological Office, well known for his invention of a sounding machine ; at Boston, from typhoid fever, W. W. Norman, professor of biology in the University of Texas ; Dr. Carl Schönlein, assistant in the zoological station at Naples, aged 40 ; Dr. Thomas O. Summers, professor of anatomy at the St. Louis College of Physicians and Surgeons, on June 19 ; Mr. Lawson Tait, the eminent surgeon, on June 13, in his 55th year, one of the earlier investigators of digestion in insectivorous plants ; Gianpaolo Vlacovich, professor of anatomy at Padua, Italy ; Prof. E. G. Balbiani, professor of comparative embryology in the Collège de France, well known for his work on the development of insects, the conjugation of Protozoa, the rôle of the nucleus, and in many other departments ; on August 16, Prof. R. W. Bunsen, F.R.S., the illustrious Heidelberg chemist, in his 88th year ; on August 1, John Cordeaux, of Great Cotes-house, Lincolnshire (born 1831), a keen ornithologist, who helped not a little to organise a systematic study of bird-migration ; on August 9, Sir Edward Frankland, the famous chemist (born 1825) ; on July 18, at Springfield, Ohio, Prof. H. R. Geiger, formerly of Wittenberg College, and lately connected with the U.S. Geological Survey ; on July 16, W. P. Johnson, LL.D., President of Tulane University, New Orleans, and a regent of the Smithsonian Institution ; Mrs. Elizabeth Thompson, of Stamford, Conn., a liberal patron of science, founder of the Elizabeth Thompson Fund for the promotion of scientific research.

[1] *Arbeit. zool. Inst. Univ. Wien*, xi. 1899, pp. i.-xii.

NEWS.

THE following appointments have recently been made :—Mr. A. F. Stanley Kent, as professor of physiology in University College, Bristol; J. L. M'Intyre, as lecturer in comparative psychology in the University of Aberdeen; C. F. Marbut, promoted to full professorship of geology in the Missouri State University; Dr. R. Martin, as professor of physical anthropology at Zürich; J. L. North, as curator of the Museum of the Royal Botanic Society at Regents' Park; Dr. A. Philippson, privat docent in geography at Bonn, to the title of professor; Dr. W. Somerville, professor of agriculture at the College of Science, Newcastle-on-Tyne, to the new professorship of agriculture in Cambridge University; Dr. E. H. Starling, F.R.S., to the Jodrell professorship of physiology in University College, London, in succession to Prof. E. A. Schäfer now of Edinburgh; Dr. E. V. Wilcox, lately professor of zoology in the University of Montana, to a position in the Agricultural Department at Washington, where he will have charge of the zoological items in the *Experiment Station Record;* Miss H. V. Whitten, as tutor in geology in the University of Texas; Mr. D. L. Wilder, as assistant on the Iowa Geological Survey; Mr. J. H. Burkill, M.A., as Principal Assistant in the Directors' Office, Royal Gardens, Kew.

The Duke of Bedford has been elected president of the Zoological Society of London in place of the late Sir William Flower.

The degree of LL.D. has been conferred by the University of Glasgow on Mr. R. L. Jack, Government geologist of Queensland.

The degree of LL.D. has been conferred by Clark University on Professors Boltzmann, Picard, Mosso, Ramon y Cajal, and Forel, who lectured at the recent decennial celebration.

Prof. K. von Zittel has been elected president of the Munich Academy of Sciences.

Surgeon-General Sir J. Fayrer, author of the "Thanatophidia of India" and other works on snakes, has had a pension of £100 per annum conferred upon him for distinguished service.

The Baly Gold Medal of the Royal College of Physicians of London, for distinguished work in physiology in the two years preceding the award, has been awarded to Prof. C. S. Sherrington.

Prof. J. Wiesner, the well-known botanist of Vienna, has been elected a member of the Berlin Academy of Sciences.

Dr. Maxwell T. Masters, F.R.S., the well-known author of "Vegetable Teratology," etc., has been made an officer of the Order of Leopold by the King of the Belgians.

Prof. Purser's work as a teacher of physiology for the last twenty-five years at Trinity College, Dublin, is being gracefully recognised by his former pupils, who are raising funds for a "Purser medal," which will be awarded annually to the candidate showing greatest proficiency in physiology and histology in the professional examination.

It is announced in *Science* that the Berlin Academy of Sciences has given Prof. Engler a grant of 4000 marks for his botanical work.

Dr. Charles Drury Edward Fortnum, who died on March 6, left the bulk of his estate, valued at £41,247, and his collections to the Ashmolean Museum of Oxford.

Mr. George Averoff, who died at Alexandria on July 27, has bequeathed £20,000 to create an agricultural school in Thessaly, and £50,000 to the Polytechnic schools and Odeon at Athens. Among his other bequests is one of £40,000 for the revival of the Olympic games, to which he devoted a similar sum in 1896.

Science announces the following gifts and bequests:—The Medical School of Harvard University is said to have received over $100,000 by the will of the late Lucy Ellis of Boston. The California Academy of Sciences has received from Mr. J. W. Hendrie securities to the value of $10,000, which will go to form a publication-fund. By the will of the late Frau M. Jankowska of Warsaw, the Academy of Sciences at Cracow receives 20,000 roubles. O. Hölterhoff, a banker, has bequeathed about 1,000,000 marks to the University of Bonn.

The supplementary vote of £65,000 required to bring about the housing of the University of London in the Imperial Institute having been agreed to, and the formal concurrence of the parties concerned having been obtained, the *matt* problem of structural adaptation is now being considered.

The University Court of St. Andrews has adopted a scheme for training candidates with a view to the Indian and Home Civil Services, which have again been brought more within the reach of Scottish students by the recent raising of the age limit. Lecturers in political economy, Sanskrit, ancient history, political philosophy, etc., have been, or will be, appointed.

We quote from *Science* the following interesting note:—Twenty-two per cent of the professors in the German universities are engaged in lecturing or laboratory supervision 2–6 hours a week, and fifty-one per cent from 7–12 hours. Of the associate professors sixty per cent are engaged from 2–6 hours per week, and of the privat docents eighty-two per cent. Only four per cent of all privat docents are engaged in lecturing or laboratory supervision more than 12 hours a week. This relative leisure may account in part for the great amount of research work done in German universities.

The summer meeting of University Extension Students at Oxford in August was attended by about 1000 students, including about 180 foreigners; and University Extension work in England is reported to be prospering.

Science notes that during the past summer session there were 4997 students matriculated at the University of Berlin, 349 more than in 1898, and including 655 foreigners.

It is reported that the number of candidates last July for the Bachelor's degree in Science was, for the first time in the history of the University of London, much greater than the number presenting themselves for examination in Arts. This interesting change is attributed to the increasing demand for science teachers in schools and colleges.

In a letter to the *Times* of August 15, Professor Raphael Meldold expresses the views of many interested in the advancement of scientific education when he calls attention to the real danger involved in the inadequate representation of science and of scientific interests among those in authority. "If the direction of the science teaching in secondary schools is at this crisis allowed to fall into wrong hands the progress of the country will be retarded for generations."

It is announced that at the seventy-first meeting of German naturalists and physicians at Munich (September 17–23) lectures will be given by Dr. Nansen on the results of his expedition, and by Prof. Chun on the German Deep Sea Expedition. Profs. Marchand and Rabl will discuss the relation of pathology to embryology.

On August 9 Professor V. Pritchard opened the International Otological Congress with an inaugural address on the history and recent advances of otology, and the retiring president, Prof. Grazzi of Florence, also gave an address.

At the annual meeting of the Royal Botanic Society on the 10th, the Duke of Teck was re-elected president. The number of new fellows and members joining during 1898 was 108, and since the beginning of this year 165 have been elected. The total number of fellows is 2102, but the society is reported to be still struggling against the common malady of too small an annual income.

The second annual "Summary of Progress" of the Geological Survey records the revision and extension of the maps of various districts. With regard to results, special attention is directed to the researches among the younger granites of the Highlands, the numerous Cambrian fossils found in Skye, the discovery of more new fishes in the Upper Silurian rocks of Lanark and Ayrshire, the evidence of the existence of volcanoes in Somerset belonging to the time of the Carboniferous Limestone, the new light thrown on the structure and probable extension of the North Staffordshire coalfield, fresh information as to the volcanic history of the western mainland of Scotland and the Inner Hebrides, and further data as to the successive stages of the Ice Age.

On Saturday, September 9, the Geologists' Association makes an excursion to Charlton, Erith, and Crayford, and on September 11 to the British Museum, Jermyn Street Museum, and Natural History Museum.

Dr. L. L. Hubbard has resigned his position as state geologist of Michigan.

We learn from *Science* that the excursions of advanced students of natural science, *e.g.*, at present of geological students, to Arizona and New Mexico, are reckoned as a regular part of the University work in Chicago.

We learn from *Science* that the State Zoologist of Minnesota, Prof. H. F. Nachtrieb, has equipped a house-boat for the study of the fauna of the Minnesota and Mississippi rivers.

The Russo-Swedish Scientific Expedition to Spitzbergen has established winter quarters at Horn Sound. Later on they will proceed by land to the western side of the Stor Fiord where they will engage in geodetic work.

The *Belgica*, with the members of the Belgian Antarctic Expedition on board, left Buenos Ayres for Europe on August 14.

Henry G. Bryant of Philadelphia, who led a search party for Lieut. Peary a few years ago, is about to attempt an ascent of Mount Assiniboine.

Prince Johann Lichtenstein has given the Vienna Academy of Sciences 25,000 florins for explorations in Asia Minor.

The Arctic Club of America, we are told by *The Scientific American*, was organised in New York in 1894, with Prof. W. H. Brewer as president, to promote a live interest in Arctic matters and to disseminate accounts of the results of expeditions. "The club has a banner of its own, which is now being borne toward the North Pole by Lieut. Peary, Walter Wellman, and others."

The slightly cracked specimen of the egg of the Great Auk sold by auction in July at Stevens's Rooms in London realised 300 guineas; a carefully made model should cost under three shillings.

At a meeting of the Royal College of Physicians on July 27 the president awarded the Bisset Hawkins Gold Medal to Dr. James Burn Russell, M.D., LL.D. Glasg., medical adviser of the Local Government Board of Scotland, and late medical officer of health for the city of Glasgow. This is the first award which has been made of this medal, which was founded in 1896 in memory of the late Dr. Francis Bisset Hawkins, to be given triennially to a medical practitioner, being a British subject, who has during the preceding ten

years done such work in advancing sanitary science or in promoting public health as in the opinion of the College deserves special recognition.

The structural alterations which have to be made at the Imperial Institute in order to provide a home for the University of London will cost £7000. The Treasury minute showed, among other arrangements detailed in it, that the Government would pay off the mortgage of £40,000 on the Institute building, and also discharge the floating debt of £15,000. This accounts for £62,000 of the vote of £65,000 which was agreed to. The remaining £3000 is for the half-year's maintenance and repairs, with fuel, lighting, and necessary furniture.

Dr. Henry Woodward of the British Museum (Natural History) has been granted an additional term of service for two years by the Treasury. This dates from October next, and is the second time Dr. Woodward has been so privileged.

The glazing of the great sauria in the gallery of fossil reptiles at the British Museum (Natural History) is now fast approaching completion. The space gained by the alteration made in this gallery is considerable, and with the exception of the upper four feet no trouble is caused by the reflection of light. Some slight alteration of the blinds will no doubt easily make the whole perfect. Below the frames of sauria is a bare space of some few feet, and this we presume will be utilised for table cases in the early future.

The Geologists' Association of London issued the usual annual pamphlet in connection with the long excursion to Derbyshire. This consisted of advance copies of the number of the *Proceedings* which will be issued at the end of the month, and forms one of a series of valuable treatises on the local geology of this county. The district dealt with includes the north and north-west portions of Derbyshire, and roughly coincides with the whole of the High Peak Division and the northern half of the Western Division of the county. The subjects included are mountain limestone, Yoredale rocks, millstone grit, sands and fire-clays, glacial drift, infas, igneous rocks, and there is a special chapter on petrology. Mr. H. H. Arnold Bemrose is author, and was also principal director of the excursion; the pamphlet can be had from the secretary for the usual eighteenpence.

CORRESPONDENCE.

DEAR SIR—I shall be glad if you will correct an impression which may be conveyed by a partial quotation from the evidence of the Select Committee of the Cape Parliament on Trawling, and appearing in a recent number of your valuable paper. The full quotation is: "the *evidence*" (*i.e.* of the fishermen examined) "has shown that we know absolutely nothing about the spawn of the fish, or very little."—Yours truly, J. G. F. GILCHRIST.

DEPARTMENT OF AGRICULTURE, CAPE OF GOOD HOPE,
CAPE TOWN, 21*st June* 1899.

[We regret that our colleague who reviewed the paper referred to appears to have misunderstood the sentence.—ED. *Nat. Sci.*]

Natural Science

A Monthly Review of Scientific Progress

October 1899

NOTES AND COMMENTS.

The Scientific Spirit.

In his eloquent presidential address to the British Association at Dover, Sir Michael Foster raised an interesting question when he inquired into the characteristics of the scientific spirit. It was after reminding his audience of the great strides in natural knowledge since 1799, and of the resulting increase in man's mastery over nature, that he roused expectation by pausing to inquire whether all this has had any effect on the mind itself. The scientific spirit has been developed, he allowed, but what is this scientific spirit?

Surely the learned Professor must have thought that his audience could not stand much psychology, for his treatment of the interesting problem which he raised was easy-going. He pointed out that the features of the fruitful scientific mind were in the main three—truthfulness, alertness, and courage. To the objection that these qualities are not the peculiar attributes of the man of science, but may be recognised as belonging to almost every one who has commanded or deserved success, whatever may have been his walk in life, he answered that this was exactly what he wished to insist—that the men of science have no peculiar virtues, no special powers, being ordinary men with characters common, even commonplace. Science, as Huxley said, is organised common-sense, and men of science are common men, drilled in the ways of common-sense.

This may be true enough—and it speaks volumes for the candour and tolerance of the British Association that such plain-speaking was even applauded—but it was none the less an evasion of an interesting problem. What we wished was an analysis of the intellectual qualities of the scientific mood. It may have been useful to point out that science is not something *per se*, apart from other intellectual products, and that the scientific mood is germinal, at least, in most healthy people, but it would have been interesting if Sir Michael Foster—as one of the most scientific men in Britain—had told us what differentiates the mood expressed in, for instance, his "Text-Book

of Physiology" from that expressed in Green's "Prolegomena," or in Newman's "Sermons," or in Whistler's "Gentle Art," or in Meredith's "Ballads of the Earth." Altogether apart from subject-matter, the intellectual note of these is quite different from that which characterises the immortal text-book referred to, and what we wished was that the Professor had told us what his particularly well-marked differentiating feature—obscured by the word "scientific"—really meant.

Much more satisfactory was the concluding part of the address, in which the President discussed the solidarity and internationalism of science. "The man of science," he said, "cannot sit by himself in his own cave, weaving out results by his own efforts, unaided by others, heedless of what others have done or are doing. He is but a bit of a great system, a joint in a great machine, and he can only work aright when he is in due touch with his fellow-workers. If his labour is to be what it ought to be, and is to have the weight which it ought to have, he must know what is being done, not by himself, but by others, and by others not of his own land and speaking his tongue only, but also of other lands and other tongues." That this is being increasingly recognised is made evident in many ways—by international congresses and bibliographies, by international co-operation in great enterprises like the Antarctic Expedition, and in smaller endeavours like the production of *Natural Science.*

More Pleurococcus.

ANOTHER filament-forming Alga, to which its discoverer, Miss Snow (*Annals of Botany,* vol. xiii. No. 4, p. 189), has provisionally given the name *Pseudo-Pleurococcus,* has been separated from the aggregate of small unicellular green forms, so long known under the collective name of *Pleurococcus vulgaris.* The new form differs in the unicellular state from the true *Pleurococcus vulgaris,* which we are glad to see Miss Snow still recognises as a constant non-filament-forming species, by the possession of a pyrenoid and of a lateral aperture in the chloroplast, while it has the power of forming filaments when grown in certain nutritive solutions.

It appears also to be distinct from the filamentous form of *Pleurococcus* described by Chodat, in which the pyrenoid was absent, and which could not be distinguished in the unicellular state from the true *Pleurococcus.* In truth, the layer of green unicellular organisms so frequently met with on the bark of trees, etc., seems to consist, not of a single polymorphic species, but rather of a considerable number of real species, which may be isolated from one another only by the employment of certain modifications of the well-known methods of bacteriology, especially by rigid attention to the sterility of cultures.

Asexual Nuclear Fusions.

Fusion of nuclei, whether it accompanies the union of so-called sexual cells, or occupies a position in the life-history which apparently denies it that dignity, must for some time remain a subject of absorbing interest, not only on account of its complexity, but also owing to the important biological questions involved.

Professor Percy Groom draws attention in a recent paper (*Trans. Bot. Soc. Edin.* 1898-99, pp. 132-144) to the number of such fusions of other than a distinctly sexual character, which we now know to occur in the vegetable kingdom.

Among fungi, in the Uredineae and Ustilagineae, the union takes place in the teleutospore, which, originally binucleate, contains but one nucleus at the period of germination, when it gives rise to the short sporidium-bearing promycelium. In Proto- and Autobasidiomycetes the fusion takes place in the homologue of the teleutospore, viz. the young basidium, which, when mature, represents, according to Brefeld, the Uredine promycelium, and bears basidiospores. Finally, in Ascomycetes the same phenomenon may be observed in the young ascus, which de Bary regards as a reduced sporophytic generation parasitic on the parent plant. Apart from fungi similar nuclear fusions are only known to occur among Angiosperms, where the union of two polar nuclei in the embryo sac precedes the formation of the endosperm, which, by the way, we are pleased to see the Professor regards as homologous with that of Gymnosperms, and consequently with the prothallus of the lower forms, its appearance having been postponed owing to functional degeneration. These fusions are thus always interpolations, and distinctly asexual in character, as is shown by the position they occupy in the life-history of such forms as the Ascomycete *Sphaerotheca* and the Angiosperms, in both of which the union takes place along with and subsequent to a well-marked sexual act, viz. the union of the antheridial and oogonial nuclei in the former, and that of the nuclei of the pollen-tube and egg-cell in the latter. In every known case they take place in a portion of the life-history, which has undergone degeneration, and which is at the same time fructificative in development, as well as frequently parasitic in character and sometimes at least homologous with the host plant (?).

Professor Groom suggests that if this fusion can be taken as evidence of vegetative degeneration in one segment of the life cycle, it may be possible to employ it as a means of distinguishing between antithetic and homologous alternation of generations among plants; but whatever be the physiological rationale of such fusions—and an adequate explanation seems still far to seek—they appear to have much in common with the similar phenomena which constantly accompany the union of sexual cells, and both will in all probability be ultimately found to perform similar functions in the life of the plant.

Inheritance of Malformations.

The inheritance of monstrous characters is a subject the examination of which may be expected to shed increased light on many important and still obscure questions, though it has hitherto failed to receive the attention it deserves. In a recent paper (*Revue Générale de Botanique*, April 1899, pp. 136-151) Hugo de Vries describes the results of a series of experiments, which he has for several years successfully carried on with regard to the inheritance of accidentally acquired fasciations in wild plants. By means of rigid selection and isolation of the parents, followed by careful cultivation of the offspring, he has been able not only to transmit the peculiarity through several generations, but even to increase the degree of fasciation. On the other hand, the tendency to reversion appears to be very strong, and notwithstanding the closest attention the resulting races never attain the permanency of those ornamental varieties so commonly cultivated in gardens. The plants examined were all facultative annuals, that is, species which are capable of giving rise to both annual and biennial individuals, and the differences between these are of some interest, if difficult of explanation. The annual forms, for example, never show fasciation till late in the season, and the malformation is confined to the upper part of the flowering stem, while those stems which spring in the second year from fasciated rosettes are fasciated throughout their whole length, and the malformation is more marked than in those of only annual duration, though even in these it may be considerably increased by early sowing under glass, or by any other method of cultivation which tends to increase the vigour of the young plant previous to the formation of flowering stems.

The Nucleolus in Heredity.

The nucleolus has hitherto played with becoming dignity the somewhat passive part of a spectator in the nuclear quadrille, but Mr. H. H. Dixon (*Annals of Botany*, vol. xiii. June 1899, p. 269) has in these latter days dragged it from its inglorious repose, and it must now share the labours of the chromatin as a carrier of the hereditary substance. During division the chromosomes perform their accustomed task, but as soon as the cell enters a resting state the hereditary substance is divided between the newly formed nucleoli and the chromatic filament, the former taking the dormant idioblasts, which are not required for the functional development of the individual cell, while the remainder are left in the chromatin. On this hypothesis the apparent absence of the reducing division in higher plants is accounted for by supposing that the necessary elimination of excessive

germ-plasm is brought about by the extrusion of nucleoli, while the deficiency of chromatin, so often remarked in the nuclei of mature specialised cells, as compared with the large size of their nucleoli, would be a natural consequence of a reduction in the number of active, and an increase in the number of dormant, idioblasts, which might be expected to accompany specialisation if, as seems probable from the phenomena of vegetative regeneration, every mature cell must contain all the hereditary substance required for the development of an individual.

Inheritance of Longevity.

WALLACE, Weismann, and others have suggested that the normal length of life of organisms, which differs so much in different species, has been determined by natural selection. A creature lives as long as is good for the species. This was a general suggestion—prompted partly by the strange irregularity and apparent capriciousness of the length of life in different animals—and the preliminary question was not raised, "Is longevity a heritable character?" This is obviously a very important question, since natural selection could not determine or fix the fit duration of life unless that character were inherited. We are indebted to Miss Mary Beeton and Professor Karl Pearson for a contribution towards the required answer. In a paper entitled "A first study of the inheritance of longevity, and the selective death-rate in man," read before the Royal Society of London on 15th June, the authors show that directly and collaterally duration of life is certainly inherited in the male line in man. They believe this to be the first quantitative measure of the inheritance of life's duration. Further data for the inheritance of this character in the female line, and for the study of the inheritance of "brachybioty," or short-livedness as distinguished from longevity, are being collected. The inquiry should be interesting to actuaries as well as to biologists.

The second part of the paper is not less important. "In the presidential address at the Oxford meeting of the British Association we were told that no one had seen natural selection at work. In a criticism then published by one of us, it was suggested that every one who had examined a mortality table had seen natural selection at work. . . . All individuals die, but some, better suited by their constitution and characters to their environment than others, survive longer, and so are able, or better able, to reproduce themselves, and to protect for a longer time their offspring. To assert that natural selection does not exist, is to assert that the whole death-rate is non-selective, or is not a function of the constitution and characters of the individual. Looked at from this standpoint the existence of natural selection really becomes a truism. All that remains when we

desire to see it at work is to determine the relative amounts of the selective and non-selective parts of the death-rate for individuals living under the like environment. If, therefore, individuals living under much the same conditions are dealt with, the determination of the selective and non-selective death-rates is a measure of the quantitative amount of natural selection."

One method of dealing with the problem has been followed by Professor Weldon, who selected a certain structural part (in crabs), and sought to determine whether the death-rate is a function of the dimensions of this part. Another method has been followed by the authors. "We do not attempt to select any organ whatever, but select individuals having any general resemblance in their constitution, or in the whole complex of organs and characters, and correlate their fitness for surviving. Now relations or members of the same family are precisely such individuals. If there were no selective death-rate, there would be no correlation between the ages of death of, say, brothers. If there were no non-selective death-rate, we ought to find that the correlation between ages of death of brothers takes the value determined for the coefficient of heredity in brothers, *e.g.* the ·4 of stature, fore-arm, cephalic index, eye-colour, etc. Actually we find it to be something sensibly less than ·4. Our investigation shows that, in round numbers, about 80 per cent of the death-rate is selective in the case of mankind. To that extent natural selection is actually at work."

The authors close the abstract of their interesting preliminary paper with an appeal for biological experiment. "Various types of life ought to be submitted to ordeals of a kind like to those which occur in nature, and the correlation between the powers of resistance to these ordeals existing in members of the same family or brood determined. We shall thus be able to ascertain under a variety of circumstances the relative proportions of the selective and non-selective death-rates. . . . One may venture to express the hope that in a comparatively few years, if enough workers can be found for the experimental side of the subject, we shall no longer hear natural selection spoken of as hypothetical, but rather its quantitative measure given for various organisms under divers environments."

A Verbose Vitalist.

NATURALISTS of an earlier day would probably be surprised—if not shocked—at some of the contents of modern biological journals. We refer to the now frequent occurrence of pages thickly strewn with equations and mathematical symbols, of others bristling with "categories" and "principles," of others where the author seems at first to be living in

another world peopled by strange creatures called biophors and determinants, and worse. These things do not of course surprise or shock us, for we have realised the value of the statistical study of variations, the need of keeping on good terms with philosophy, and that Weismann's symbols are "not mere fanciful images, but realities," as he says, "in the same sense in which chemical atoms and molecules are realities." We are not surprised at these papers; what surprises us is that so few people seem to read them. A fragment of skin from a Patagonian cave seems to excite more interest than one of Karl Pearson's mathematical contributions to the study of evolution; the problem of trituberculy is familiar, but Mr. Sandeman's "Problems of Biology" remains unheeded; discussions of mimicry abound, but we might almost count on our fingers the English references to Weismann's essay on Germinal Selection. Is it that we have forgotten our mathematics, is it that we have become after many lessons "philosophie-scheu," or is it that our love of the concrete is too strong? There are these and other reasons on our side, but it must be allowed that the fault is not wholly ours. It is certain that one reason why contributions to the philosophy of biology are so frequently disregarded, is the author's low standard of lucidity. Enigmatical sentences, tense with meaning, may be gloated over if they are written by Browning, but not if they come from a biologist. Aphorisms which sound as if they meant much (as they probably do), which seem, however, only successful in keeping their meaning hidden, may be entertaining in a novel by Meredith, but they are only irritating in an essay on morphogenesis. Thus, through the carelessness of authors and the busy preoccupation of readers, we are left to continue our work but slightly influenced by the constantly growing mass of occult biological literature. We know of a prominent worker who bundled up one of these voluminous riddles, labelled it "Davidson's Secret," and threw it on the top shelf; and we quite sympathise with any busy biologist who should similarly treat the little book before us. It is called "Die Lokalisation morphogenetischer Vorgänge. Ein Beweis vitalistischen Geschehens" (Engelmann: Leipzig, 1899, pp. 82, 3 figs.). It might be flippantly called "The Mystery of Hans Driesch."

It was begun, we are told, at San Martino de Castrozza 9 ix. 98, finished at Naples 19 xi. 98; and it was originally published in the *Archiv für Entwickelungsmechanik der Organismen.* Its importance, we read, lies in the fact that it not merely suggests but proves the necessity of recognising a new and peculiar orderliness (Gesetzlichkeit) in certain vital phenomena. It contains a proof of vitalism. And by vitalism is here meant the recognition of the unique character of organisms, the recognition of what transcends the categories of mechanism,—"diejenige Auffassung, welche in Lebensgeschenissen Vorgänge mit ihnen eigenthümlicher Elementargesetzlichkeit erblickt."

The key-note is in the word "localisation." It is especially the

"localisation" of developmental processes which appears to the author to bring out clearly the distinctive character of an organism as opposed to an inanimate system. The first illustration given may make the matter plainer.

Some four years ago Driesch showed that if a fully-formed gastrula of a sea-urchin (*Sphaerechinus granularis*) be halved equatorially, so that each half has half of the ectoderm and half of the archenteron, both portions heal up and become spherical again, and both soon show a gut divided in the normal proportions into three parts. This is a simple instance of a familiar kind of phenomenon which appears to the author to prove the necessity of vitalistic interpretation. No chemico-physical interpretation will suffice.

We cannot here summarise the author's argument, not that it is particularly difficult—for Driesch's style is limpid compared with that of many—but because of the difficulty of translating the terminology. It may be all right in German and in Germany, but we doubt if the conversion of English biologists is likely to be attained by discussions on "Der primär-regulatorische Charakter der Differenzirung harmonisch-äquipotentieller Systeme," and the like. The little book was written in about two months; it seems to us that in this, and even more in other cases, it would have been well if the author had spent an equal amount of time in making the wisdom of his counsel more generally available to busy biologists.

To return for a moment to the subject-matter. The machine theory of an organism is insufficient, since some of the most characteristic vital phenomena seem to transcend the categories of mechanism. And even if we come to understand a living creature as we understand a steam-engine, there remains the idea behind them both. Sooner or later we have to fall back upon an unknown "Gesetzlichkeit." The author's contention is that there is in the organism an elementary irreducible "Gesetzlichkeit." To overlook this, he says, is like overlooking the spider in our science of the web.

Morphology of the Sting in Hymenoptera.

THE embryological researches of the last twenty years seem to have securely established that the stinging apparatus in ants, bees, and wasps is derived in part from ventral segmental outgrowths, and in part from the integumentary skeleton of certain segments. In a recent paper (*Zeitschr. wiss. Zool.* lxvi. 1899, pp. 289-333, 2 pls.) Dr. Enoch Zander has analysed the apparatus in a number of representative forms, and has shown in detail how much of it is referable to (the 11th and 12th) segments of the abdominal skeleton, and how much to the genital appendages or gonapophyses. He shows further

that the latter are not developed until the larval stage is reached, and are therefore in no wise comparable to the abdominal appendages which appear and disappear during the strictly embryonic period. In fact, he confirms the conclusion of Heymons that the leg-rudiments and the gonapophysal rudiments are in their nature quite distinct.

Factors in the Growth of Muscle.

We have previously noticed Mr. Alexander Meek's interesting conclusion that in the post-embryonic history of striped muscles in various mammals (cat, sheep, field vole, white rat) there is a reduction in the number of fibres accompanied by a considerable hypertrophy of the survivors. Dr. B. Morpurgo got a different result in examining the white rat, and Mr. Meek briefly answered him, maintaining his position that there really is in the history of a muscle "a struggle of parts within the organism," and a resulting "survival of the fittest."

In a more recent paper (*Journal of Anatomy and Physiology*, xxxiii. 1899, pp. 596–608) he discusses the question in greater detail, and as the subject is one of much practical and theoretical interest, we quote his summing-up. "The life-history of muscle seems to be determined by (1) inherited qualities, present in the fertilised ovum, the evolution of which is controlled by (2) internal influences—internal secretion (including the effects of 'sex'), the mutual influence of the muscles upon one another, and of the fibres upon one another, and the internal variations amongst the fibres; and by (3) external circumstances—work, food, habit, and indeed, the ordinary and extraordinary conditions of extra-uterine life."

"Up to the time of birth, in at any rate the higher mammals, perhaps in all the Eutheria, hyperplasia characterises the growth of muscle; while after or about birth, hyperplasia ceases, and extra-uterine life brings about a selection of some of the fibres at the expense of their neighbours. In other words, during extra-uterine life, muscle, according to its position, suffers more or less a reduction in the number of its fibres, the degree of which is expressive of its functional importance. The surviving elements are at the same time greatly hypertrophied, and the extent to which this takes place is also expressive of the work which the muscle performs, or of which it is capable."

Water-Plants as Land-Winners.

In *The Naturalist* for August Mr. Albert Henry Pawson makes a brief contribution to the study of the influence of water-plants on the

land surface. "There are several ways in which these plants tend to diminish the water-space and to increase the dry land. By their own decay they form vast masses of vegetable soil in shallow waters and on water margins; by occupying running streams they moderate the flow of the current and give it time to deposit its silt; by their creeping rhizomes and spreading roots they fix the bed of a stream and prevent it being scoured and deepened by floods, and again in times of flood they serve as a sieve or strainer, arresting all floating and much suspended solid matter." This is indeed a familiar theme, but the author discusses it with freshness and with appreciation of its dramatic interest. . . . "Inch by inch, as the result of this accumulation and decay, the land creeps in upon the mere; more and more solid grows the edge; the aqueous plants retreat from the now shallow margin, the terrestrial plants advance, finding firmer footing; the sedges and reeds crowd on their floating neighbours which need space, and cannot endure the shade; these, too, press forward, and the open water grows less and less; it is invested on every side, and it is plain that its complete subjugation is now only a matter of time." It would be of interest to procure some actual measurements of the amount and rate of land-winning, and to study in minute detail the elimination which proceeds as the mere is closed up.

The Progress of a Great Work.

EIGHT parts are now available of "Das Tierreich"—the "Systema Naturae" up to date—which is being issued to an ungrateful world by the German Zoological Society through the medium of R. Friedländer and Son in Berlin. The *magnum opus* will give a classification and diagnosis of all living animals, and the issue of eight parts in a relatively short period permits us to hope that we shall live to see it completed. The general editor is Professor Franz Eilhard Schulze, and there are many sub-editors. Of the collaborateurs whose names are published the majority are German, but most of the European countries are represented by well-known workers. Britain is represented by Mr. W. E. Hoyle of Manchester, the Hon. L. Rothschild, Drs. Hartert and Jordan of Tring, Mr. A. D. Michael, Mr. W. R. Ogilvie Grant, and Dr. Bowdler Sharpe in London, the Rev. T. R. R. Stebbing in Tunbridge Wells, and Prof. D'Arcy W. Thompson in Dundee. The part before us is by Dr. A. Labbé, and deals with the Sporozoa; it occupies 180 pages, has 196 figures, and costs 8·80 marks to subscribers, and about a third more if purchased singly. The other parts published deal with various families of birds and mites, with a division of copepods, and with scorpions and Pedipalpi. It is not necessary to point out the magnitude of the boon which this great work will confer on systematic zoology, but perhaps it is permissible to urge individual

workers to purchase the separate parts which interest them. A subscription to the entire work is too much to expect, except from Universities, Museums, learned Societies and the like; and even some of these seem slow to recognise that the purchase is a duty. We are told, for instance, that from one of our famous university towns, with libraries, museums, and rich colleges, no single order for "Das Tierreich" has as yet been received. What an ungrateful world it is.

The Hopkins Seaside Laboratory.

In the *American Naturalist* for August, Professor Vernon L. Kellogg gives an account of the Hopkins Seaside Laboratory of the Leland Stanford Junior University. It is situated on the bay side of the promontory Point Pinos, which is the southern limit of the Bay of Monterey. In addition to a fauna more or less peculiar to itself, the bay contains a number of sub-tropical and sub-boreal types peculiar to the north and south zones of the Pacific coast between which it lies. "A well-known and experienced biologist of the University of Chicago, who spent a summer at the Hopkins Laboratory, has said that Monterey Bay and the Bay of Naples are much alike in the abundance and representation of species," and the laboratory has this in common with the Naples Station, that it can be used to advantage at any time in the year. The regular sessions for students are in June and July, and the fee is twenty-five dollars. Investigators prepared to carry on original work may use the laboratory and its equipment free of charge, and seventeen private rooms are placed at their disposal.

The Morning of Science.

It was a momentary aberration which led a great zoologist—recently lost to science—to suggest, in the enthusiasm of a retrospect, that it was now time for us to be making a list of the things we did *not* know. A very different suggestion is conveyed in a remarkable sentence in the presidential address delivered by Dr. Edward Orton at the meeting of the American Association for the Advancement of Science. After following Mr. Alfred Russel Wallace in a retrospect of the progress of science, the President pointed out that the very title of the Association indicated that the work of science was far from complete. "The founders of the Association, fifty years ago, clearly saw that they were in the early morning of a growing day. The most unexpected and marvellous progress has been made since that date, but as yet there is no occasion and no prospect of modifying the title.

We are still labouring for the advancement of science, for the discovery of new truth. The field, which is the world, was never so white unto the harvest as now, *but it is still early morning on the dial of science*." The address was not a remarkable one, but we commend this last sentence to the attention of those who speak as if it were already late afternoon.

Eruption of Mauna Loa.

In the *American Journal of Science* for September some account is given of the beginning of an eruption of the volcano of Mauna Loa, on Hawaii.

Early in the morning of 4th July, one observer says, "an immense column of smoke and steam was seen rising from the crater of Mokuaweoweo. It was pierced through with the light from the fires beneath, until it glowed and shone like a column of fiery light, resplendent beyond description, and reflecting its burning glow over the whole heavens. The column seemed to be at least five miles in diameter, and rose to a tremendous height. On Tuesday the column of fire had disappeared. In place of it was the equally impressive glow of the lava as it broke from the lower side of the crater several thousand feet lower down than the column of light had been, and was thrown upward to a wonderful height by the forces which were in action. On either side of the stream, whose surface of fiery red could be seen like a line of glowing molten metal, were two cones which had formed since the eruption began. It was from these that the lava was being ejected. It was thrown up in fiery cascades high in the air. These cascades, in falling, built up the cones, and the molten lava running off from these formed the stream flowing off towards Hilo. It would be hard to say how high these cones were, perhaps somewhere between 500 and 1000 feet high, and half a mile in diameter, and five miles apart." A later account mentions three lava streams, one in the direction of Hilo, another off through Kau to the south-east, and a third towards the crater of Kilauea.

The journal from which we have cited the above also calls attention to a paper by Mr. C. J. Lyons, of Honolulu, on "Sun Spots and Hawaiian Volcanoes," published in the April number of the *Monthly Weather Review*. The author gives a table of the years of minimum sun spots for the past century, with the dates of prominent volcanic eruptions of Kilauea or Mauna Loa, showing a striking correspondence between the times of the two phenomena. As pointed out by the editor of the *Review*, however, a more thorough investigation is needed to prove that the coincidence noted is due to a real causal connection.

The Poison of Darnel.

THAT the darnel (*Lolium temulentum*) is a poisonous grass, is an old-established and familiar fact, and experts, at least, are aware of Hofmeister's research, which disclosed the presence of two active principles: temulin, obtained as chloroplatinate, which acts upon the nervous system, and the other, determined by the oily substances and fatty acids of the seed, which attacks the alimentary canal. A new interpretation, however, has recently been suggested by Mr. P. Guérin, of the School of Pharmacy in Paris (*Botanical Gazette*, xxviii. 1899, pp. 136, 137).

He has observed in the seeds of the darnel the almost constant presence of a fungus, to which it seems to him reasonable to assign the poisonous effects. This fungus, which is not the *Endoconidium temulentum* of Prillieux and Delacroix, has also been detected by Vogl, Hanausek, and Nestler, but Guérin has shown its general occurrence, and that not only in the darnel, but in *L. arvense* With. (a variety of *L. temulentum*) and *L. linicola* Sond. as well. Its presence in perennial rye-grass is quite exceptional. Guérin has also made the suggestion that the temulin of Hofmeister may be the result of the action of the fungus upon the nitrogenous materials in the peripheral region of the seed.

The fungus, which is always present in the form of mycelial filaments, appears at an early stage in the interior of the ovary, and invades the entire nucellus. It is afterwards crowded out by the development of endosperm after fertilisation, and comes to be restricted to the region between the hyaline layer (which represents the remains of the nucellus) and the outermost endosperm. The observer found this disposition of the fungus in material from Bolivia, Brazil, Chili, Abyssinia, Persia, Syria, Spain, Portugal, Sweden, Germany, and many localities in France. In forty seeds of most diverse origin the mycelial zone was lacking only in three.

Coppinia.

WE are glad to note that Mr. C. C. Nutting, writing in the *Proceedings of the United States National Museum* (vol. xxi.), is able to bring forward some very definite proofs that the remarkable hydroid structure called *Coppinia* is a cluster of gonangia of *Lafoëa*. It is remarkable that Nutting's investigations made upon the species *Lafoëa dumosa* from Puget Sound were carried on independently of Levinsen's investigations on *Lafoëa fruticosa* from Greenland, in which corresponding results were obtained.

Scientific Explanations.

The progress of science is continually hindered by the limitations of language. What a bugbear, for instance, has been the word "law"—an innocent metaphor to the careful, but an inhibiting fallacy to the many. For, as every one knows, the "laws of nature" were for many decades the subjects of naïve personification, and made to will and act as self-sufficing governors of phenomena, while now, as Professor J. H. Poynting remarked in his opening address to Section A of the British Association, "we can only assign to them the humble rank of mere descriptions, often tentative, often erroneous, of similarities which we believe we have observed." It is indeed a fall of the mighty.

But though the word "law" has almost ceased from troubling, there remain many others which still exert their pernicious influence. Prominent among these is the word "explanation," at which we are glad to see that Professor Poynting has also made some deadly thrusts. Thickly scattered through scientific literature the student finds what are called "complete explanations," but occasionally he is confronted with the strange remark that science does not give any explanations at all. What does it mean?

The meaning is simply that while the teleological idea (of "final cause," etc.) is essential to any attempt at a complete or philosophical consideration of facts, *e.g.* to a theory of the living organism, it is irrelevant and inhibitive in scientific inquiry, which is strictly aetiological. But let Professor Poynting speak for himself.

"We have not to go very far back to find such a statement as this—that we have explained anything when we know the cause of it, or when we have found out the reason why—a statement which is only appropriate on the psychical view. Without entering into any discussion of the meaning of cause, we can at least assert that that meaning will only have true content when it is concerned with purpose and will. On the purely physical or descriptive view the idea of cause is quite out of place. In description we are solely concerned with the 'how' of things, and their 'why' we purposely leave out of account. We explain an event, not when we know 'why' it happened, but when we show 'how' it is like something else happening elsewhere, or otherwise—when, in fact, we can include it as a case described by some law already set forth. In explanation, we do not account *for* the event, but we improve our account *of* it by likening it to what we already know. . . . The aim of explanation, then, is to reduce the number of laws as far as possible, by showing that laws, at first separated, may be merged in one; to reduce the number of chapters in the book of science by showing that some are truly mere sub-sections of chapters already written. . . . To take an old but never-worn-out metaphor, the physicist is examining the garment of nature,

learning of how many, or rather of how few, different kinds of thread it is woven, finding how each separate thread enters into the pattern, and seeking from the pattern woven in the past to know the pattern yet to come." . . . We have heard from unfriendly critics much in regard to the dogmatism of science; it is time rather to speak of its modesty.

An Unsolved Problem.

In his opening address to the Chemical Section of the British Association, Dr. Horace T. Brown not unnaturally took for his subject the fixation of carbon by plants, a problem towards the solution of which he has himself made some notable contributions. The address is a fine illustration of the true scientific temper, and of the value to biologists of co-operation with workers in chemistry and physics. Definite results are still far to seek, but the address indicates a hopeful outlook, and it also impresses us anew with the danger of hard and fast statements, and with the incipient character of vegetable physiology.

The president of Section B began by pointing out that although we cease not to impress upon our students that the higher plants derive *the whole* of their carbon from atmospheric sources, the experimental evidence for this hard and fast statement is very indirect. "There can, of course, be no doubt that the primary source of the organic carbon of the soil, and of the plants growing on it, is the atmosphere; but of late years there has been such an accumulation of evidence tending to show that the higher plants are capable of being nourished by the direct application of a great variety of ready-formed organic compounds, that we are justified in demanding further proof that the stores of organic substances in the soil must necessarily be oxidised down to the lowest possible point, before their carbon is once more in a fit state to be assimilated." Along with Mr. F. Escombe, Dr. Brown has been recently experimenting in order if possible to reach some satisfactory answer to this important question. "Up to the present time," he says, "our experiments have not been carried far enough to enable us to give a positive answer to the main question, but they have already suggested a new method of attack which will enable us in the future to determine, with a fair amount of certainty, whether any particular plant, growing under perfectly natural conditions, derives any appreciable portion of its carbon from any other source than the gaseous carbon dioxide of the atmosphere."

The address contains a valuable critical account of what has been done in the past, and we venture to quote the summing-up. It does not sound altogether encouraging, but there is no object in blinking the facts. "The brilliant discoveries of recent years on the constitution and synthesis of the carbohydrates have not brought us sensibly

nearer to an explanation of the first processes of the reduction of carbon dioxide in the living plant. The hypothesis of Baeyer (that the first act of assimilation is the reduction of carbon dioxide and water to the state of formaldehyde) still occupies the position it did when it was first put forward nearly thirty years ago, although it has, it is true, received a certain amount of support from the observations of Bokorny, who found that formaldehyde can, under certain conditions, contribute to the building up of carbohydrates in the chloroplasts. . . .

"The view which Timiriazeff has put forward, that there is a mere physical transference of vibrations of the right period from the absorbing chlorophyll to the reacting carbon dioxide and water, is, I think, far too simple an explanation of the facts. Chromatic sensitisers have been shown to act by reason of their antecedent decomposition, and not by direct transference of energy, and the same probably holds good with regard to chlorophyll, which is also decomposed by the rays which it absorbs. We must probably seek for the first and simplest stages of the assimilatory process in the interaction of the reduced constituents of the chlorophyll and the elements of carbon dioxide and water, the combinations so formed being again split up in another direction by access of energy from without.

"The failure of all attempts to produce such a reaction under artificial conditions is, I think, to be accounted for by the neglect of one very important factor. We are dealing with a reaction of a highly endothermic nature, which is probably also highly *reversible*, and on this account we cannot expect any sensible accumulation of the products of change, unless we employ some means for removing them from the sphere of action as fast as they are formed.

"In the plant this removal is provided for by the living elements of the cell, by the chloroplasts, assisted doubtless by the whole of the cytoplasm. We have here, in fact, the analogue of the *chemical sensitisers* of a photographic plate, which act as halogen absorbers, and so permit a sensible accumulation of effect on the silver salts.

"When we have succeeded in finding some simple chemical means of fixing the initial products of the reduction of carbon dioxide, then, and then only, may we hopefully look forward to reproducing in the laboratory the first stages of the great synthetic process of nature, on which the continuance of all life depends."

ORIGINAL COMMUNICATIONS.

The Influence of the Nervous System in Organic Evolution.

By R. F. Licorish, M.D.

The majority of biologists may be at present divided into two schools, Neo-Darwinian and Neo-Lamarckian, and besides these there are others who still profess to be unable to reconcile themselves to the truths of organic evolution as interpreted by either party, and who find a prominent representative in the celebrated pathologist Virchow. In addition to the above there are a few who, like the writer, are pure Lamarckians, and who, accepting the data of Lamarck, interpret them by the light of present day knowledge, and look on "natural selection" and "survival of the fittest" as mere "figures of speech," expressive of results which have been brought about by functional and environmental adaptation. Of the two leading schools the more numerous is undoubtedly that of the Neo-Darwinians, who see in natural selection an all-sufficient cause for organic evolution. The members of the other school, that of the Neo-Lamarckians, consider natural selection as merely *one* of the factors of organic evolution, another being the inheritance of the results of the organism's post-natal experiences.

Let us look more closely at these theories to see if we cannot find therein such a relationship or analogy as would lead us to believe that a slight modification in the basis of one or the other or both will tend to more harmony than at first sight would appear to be possible. For it must be remembered that both schools are represented by able and gifted men who devote themselves to experiment and observation, and are all equally eager to arrive at truth.

Taking the Neo-Darwinians first, we find the basis of their theory to be this. Organic evolution depends on, and is carried out through, the variations which appear at the conclusion of the ontogenetic development, *i.e.* at birth. This is the basis of their theory of organic evolution. To the question, What gives rise to those variations? we have as answers:—(1) Cause unknown (Darwin); (2) Chance—a

system of "trial and error" (Huxley); (3) The reaction of the germ-plasm to external stimuli, *i.e.* the reaction of the developing organism to the external environment (Weismann).

Let us now turn to the Neo-Lamarckians. The basis of their theory is that the influences of the environment modify the organism not only during the time it is being built up, but also for an indefinite period after, assuredly during the time it is reaching its maturity or full growth, that such modifying reactions are heritable, and that on these influences the progress of evolution is chiefly dependent. As a result of limiting inheritance to the reactions of the environing influences during the pre-natal period, Neo-Darwinians have to call to their aid natural selection, whereas the Neo-Lamarckians, by extending the period of inheritance of environmental and functional reaction to maturity of the organism, can dispense to some extent with natural selection, believing as they do that the experiences of the organisms from inception of life to maturity are conserved by heredity, and that adaptation results in most cases through inheritance of those experiences. As to the strict Lamarckian, he sees no need of natural selection, believing that somatic experience is the sole cause of adaptation.

Weismann, in addition to his theory of pre-natal influence as a cause of variation, has elaborated the theory that the organism is built up and comes to maturity because the germ-plasm, during the building up of the organism, becomes distributed through it, so as to form Anlagen which are capable of developing the necessary characters and of providing for lost parts, etc. It is this feature of his work as a biologist that has made him a distinctive force in the science. And although at first he maintained that the germ-plasm as present in the germinal cells is unchangeable, more recently he has modified his position, now maintaining, as already stated, that it can react to external stimuli, and hence be changed by the influences of the environment,—an admission of the utmost importance in the re-adjustment of apparently conflicting theories. We wish to suggest an interpretation of the Lamarckian theory that may bring about a still closer approximation.

It must be acknowledged by all who make a careful study of the nervous system in its relation to evolution, and in its influence on the organism, that it is through it that all functions are carried on, and through its regulation that lost parts are renewed and injuries repaired. Moreover, it is through the nervous system, presumably as germ-plasm as well as an organised portion in the ontogeny, that all experiences acting thereon are registered and transmitted to the off-spring. It has always been a surprise to me that biologists, in considering the factors of organic evolution, should have paid so little attention to the influence of the nervous system in vital processes; preferring, it would seem to me, to invest the cells themselves with

the power of reaction to the incident forces of the environment, and ignoring the desires demanding satisfaction which arise *de novo* within the brain itself. To a physician, on the other hand, the nervous system is by far the most important part of the human body. He knows that all medicines that act physiologically, and not purely chemically or mechanically on the system, do so through the nervous system. He knows experimentally that if the nerves to the organ on which a medicine acts be severed, the organ fails to respond. We know that if undue heat be applied to a portion of the surface of the body the vessels dilate and the part becomes redder, because on the heat being applied the terminal nerves telegraph to the nearest nerve-centre that help is needed to resist the irritation. Through the vaso-motor nerves controlling the calibre of the blood-vessels these dilate, probably that the increase of blood may carry off the excess of heat; the part thus making an effort to ward off injury. We may well assume that if the nervous connection were severed no dilatation of blood-vessels would take place, and in consequence the parts would suffer. Again, we know from the study of diseases that if the centre in the spinal cord for the nutrition of any special muscle be destroyed by inflammation, the muscle gradually dwindles from lack of nutrition. The whole study of pathology teaches us how, if through disease or accident defects are produced, they are remedied through the nervous influences operating correlatively on adjacent cells and tissues. Again, if a large blood-vessel be destroyed either accidentally or intentionally for purposes of cure, the small blood-vessels supplying the parts affected and anastomosing with those of adjacent parts gradually enlarge and carry on the function of the destroyed large vessel,—a fact which shows us how the distribution of blood may gradually become modified through functional change in the process of evolution of one species into another. In experiments on animals we learn that, although normally certain cells have a definite function, yet if the nerves governing those cells be severed, so that the connection between the cells and the nerve-centre is destroyed, the function of the cells ceases, and that if the centre for the nutrition of the cells be also destroyed the cells will die. Whether this is a direct result or due indirectly to the loss of nutrition has not yet been positively determined, probably it is due to the latter. Hence we can positively assert that the cells of the organism have no inherent power in themselves to exercise their function, or even to maintain their vitality, but that the nerve-centres through their connections with the cells supply that power which manifests itself as the function, and even as the vitality of the cells themselves. Thus my contention is supported, that if in the germ-cell the germ-plasm is the most important part as the bearer of the life functions, so in the finished organism the nervous system is the bearer of the like processes, commanding and controlling all life and function.

It seems to me that biologists look on the nervous system in the same light as they do other parts and organs of the system. Now, while this may be true in relation, *e.g.*, to the special senses, it must be remembered that the nervous system has also a general function, and must be looked at as belonging to and ministering to all other parts of the organism, so that the unity of all may be secured. Thus Dr. Gadow (in "The Last Link") says: "It is the physiological momentum which models the organism, and, by causing its adaptation, has produced its organs by change of function"; and again, "Each cell has a function, the more specialised the more intense it is." He attributes adaptation to the disturbance of the equilibrium of the cell, and its efforts to return to the *status quo* through increased activity. But whilst this may be true, so far as it goes, yet it is plain that Dr. Gadow ignores the influence of the nervous system, and attributes the sole power of adaptation to the cells themselves, while the foregoing remarks on the nervous system, and other facts which I shall advance farther on, go to show that the power of adaptation does not belong to the cells themselves, but to the correlative influence of the nervous system. If we restrict ourselves to the view suggested by Dr. Gadow's remarks, as in fact all Neo-Lamarckians seem to do, there is little wonder that the origin of correlative adaptations, as on the neck and other parts of the giraffe, presents a formidable difficulty, and appears almost inscrutable. The idea that life is due to some unknown and indefinable principle inherent in the cells themselves, pervades the whole of Mr. Herbert Spencer's work on "Biology," and finds its highest presentation in the writings of Virchow, so prominently brought to our notice in his recent Huxley lecture. In his chapter on the dynamic elements of life (in the "Principles of Biology"), Mr. Spencer mentions the fact that an excised liver, and in a more forcible way the excised heart, of a cold-blooded animal continues to function after detachment from the organism, but does not attribute such action to the nervous ganglia connected therewith. It must be remembered that such a continuation of function occurs, as regards the heart in particular, only in the lower organisms,[1] *i.e.* animals in which the nervous system and hence power is not so thoroughly centralised in the brain as in higher forms. In fact, there are more semi-independent ganglia dispersed through the organism. In the vegetable world we see a somewhat analogous distribution of independent centres, *e.g.* in the *Begonia*. Prof. Waller ("Text-Book of Physiology") thus writes: "Protoplasm is excitable. When any part of a lump of protoplasm is excited, the lump moves. When many lumps of protoplasm are gathered together into a homogeneous mass, excitations and movements may be transmitted from lump to lump in all directions. With higher organisation of the mass, differences of function and structure

[1] With proper precautions the excised heart of a mammal may continue beating for some time.—ED.

begin to make their appearance. Excitability, while still pervading the whole organism, becomes localised with greater intensity in some parts than in others; along some lines than along others (sense organs, nerves, and nerve-centres); in other parts contractibility becomes the salient character (muscles). To illustrate this progressive elaboration of a nervous system, we may select—(1) an amoeba; (2) a jelly-fish; (3) a frog; (4) a man." Thus we learn how gradually the nervous system is evolved, becoming, as organisation increases, more and more specialised in diversity of function, from, let us assume, invisible threads of granular protoplasm to the gray matter of the human brain, and the associated prolongations throughout the body. We must also recognise that the nervous energy is gradually diversified and intensified as evolution proceeds upward, from a mere automatic action in the protozoon, to the varied and diversified functions of man, mental as well as physical.

In his "Principles of Biology," Mr. Herbert Spencer says: "In whatever way it is formulated, or by whatever language it is obscured, this ascription of organic evolution to some natural aptitude possessed by organisms, or miraculously imposed on them, is unphilosophical. It is an assumption no more tenable than the assumption of special creation, of which, indeed, it is a modification, differing only by the fusion of separate unknown processes into a continuous process." It seems to me that, in making the above statement, Mr. Spencer wholly overlooks the power of the nervous system in rendering organisms capable of reacting to the influences of the environment. We may confidently ask, if the organism does not possess such a function, to what must we attribute the power of reaction? for unless we do recognise such a power inherent in the organism, rendering it capable of being gradually modified in relation to its needs, wants, or desires, and the incident forces of the environment, the only alternative is to believe in a power otherwise derived, *i.e.* in special creation or creations.

In addition to the evidence already adduced, I may take as an illustration of the power of the organism to respond to its needs in a definite way, Loeb's experiments to produce heteromorphosis, as cited in "The Biological Problem of To-day," by Hertwig. "In *Tubularia mesembryanthemum*, a hydroid polyp, there are stalk, root, and polyp-head. If one cut off the head, a new head will be formed in a few days, this being a case of regeneration. On the other hand, a heteromorphosis may be produced by modifying the experiment as follows:—Both root and head must be cut off from the stem; if the lopped piece of the stem be stuck in the sand of the aquarium by the end that bore the head, then the original aboral pole, in a few days, produces a head; if the lopped piece of stem be supported horizontally in the water, then each end produces a head." Hertwig goes on to give illustrations to show how, in other organisms, heads, tentacles, and

eye-spots may be induced to grow if steps be taken to initiate the changes. Here we have evidence as to how the inherent power of the organism—not the cells—may respond in a definite direction to fulfil its requirements.

In the evolution of the nervous system we must recognise two stages of development, the one gradually merging into the other. As Wilson says in his "Zoology": "In the lower or invertebrate forms of life, the nervous apparatus may be considered to be almost wholly occupied in the reception of the ordinary sensations which minister to the wants and necessities of existence, without any active or intelligent appreciation of the causes or results of the sensations thus conveyed. In the Vertebrata, on the other hand, we find the higher perfection of the correlative apparatus associated with powers which place the organism far above the rank and relations of a piece of automatic mechanism." We accordingly notice this specialisation of the correlative powers in these higher forms, evincing itself in the possession of a power of appreciation of the origin of sensations known as "intelligence"; whilst, in virtue of this latter feature, we find another and distinctive power superadded, which is devoted to the regulation of the movements of the body, and which is known as the power of "volition" or "will." Now, it is the possession of these varied and distinctive features, due in the lower orders to reflex nerve action, and in the higher to a species of intelligence, that led Lamarck to denote as needs, wants, or desires the processes through which animals satisfy the physical wants of their bodies. For whilst, in the lower, that process by which the exigencies of the organism are satisfied, may be considered as in response to a need, as in the vegetable world; in the higher, a species of will is manifested, as the will for food, etc., and this may certainly be construed as a desire.

Professor Conklin, in an able article on the factors of organic evolution (in "Footnotes of Evolution," by Professor Jordan), has arrived at some inconclusive deductions, under the head "Use and Disuse." He remarks: "I take an example which will serve as an illustration of a whole class. Jackson says that the elongated siphon of *Mya*, the long-necked clam, is due to the habit of burrowing in the mud; or, to quote his own words, 'It seems very evident that the long siphon of this genus was brought about by the effort to reach the surface induced by the habit of deep burial.' It certainly would be pertinent to inquire (asks Professor Conklin) where it got this habit, and how it happened to be transmitted. It is surely as difficult to explain the acquisition and inheritance of habits, the basis of which we do not know, as it is to explain the acquisition and inheritance of structure which are tangible and visible." That Professor Conklin does not understand the acquisition of habits shows clearly that he does not understand Lamarck. I have already in these pages explained my interpretation of the nature of functions, but my point may

be further illustrated in this way:—The functions of organisms, especially the higher ones, are divided into two sets—the first "*vital*," the second "*organic*." The "vital functions" are those of nutrition, reproduction and protection, those on which the life of the individual and the perpetuation of the species depend, and which in the higher organisms are satisfied through desire. On the other hand, the "organic functions" depend on the structure of the special organs. Thus, for instance, in respiration, the exchange of gases is effected according to the special structure of the breathing organs. Now all habits of animals are acquired through the vital functions originating, either as a reflex action as in the lower, or in response to desire as in the higher orders. Singularly enough Mr. Herbert Spencer, instead of recognising the importance of the acquisition of habits, has discussed the matter under the head "distribution." Now, whilst the distribution of animals does lead to the acquisition of new habits, it is only an indirect cause, the direct cause being the efforts made by the animals themselves to suit their life to the new circumstances. And they do this to satisfy their vital functions—in particular, that of nutrition. There are woodpeckers in the United States that feed on fruit, and Darwin saw woodpeckers in Patagonia feeding on insects in the air. How was the new habit of feeding on other than their customary food acquired? Clearly in their desire to satisfy the craving for food. Darwin also saw and examined certain birds, originally webbed, showing the beginning of web-disappearance. But, and here we see the significance of such a fact, the birds in which he saw such a beginning of web-disappearance had become habituated to another mode of life than that on water. They had then acquired a new habit of life, and through disuse the web had begun to disappear. We thus learn that the habits of animals, whether through reflex actions as in the lower organisms and as in plants, or in the higher orders in response to the desire to satisfy the vital functions on which the life and perpetuation of the species depend, are the results of the demands which the exigencies of the organism require for the satisfaction of the vital functions. These demands of the organism Lamarck clearly understood; and why, in the present day, biologists fail to consider them is a matter of surprise to me. Not to recognise them in the light in which Lamarck, and doubtless Goethe too, recognised them, renders the doctrine of organic evolution less intelligible, and thus more difficult to harmonise with other truths.

Let us now consider the question: Why, and how, are the modifications functionally produced by change of habits inherited? As already stated, I distinguish between the Neo-Lamarckians and the true Lamarckians in this way. The former believe in the inheritance of functional modifications, but only as brought about through cell-activity, thus failing to see how correlative parts are modified; whereas the secret of the true Lamarckian's position is, that he understands

both how separate parts as well as correlative parts are modified. Present-day knowledge goes to show that such changes are brought about through the co-operative influence of the correlative brain centres. Yet, strange it is that the leader of the Neo-Lamarckians, Mr. Herbert Spencer, while he cannot see how natural selection can produce such changes as are shown in the neck, etc., of the elk, has to fall back on natural selection to explain the modifications shown in the fore-quarters of the giraffe, a more difficult matter than the elk's neck to bring under the influence of natural selection. If the changes in the elk's neck cannot be explained by natural selection, how can the parts of the giraffe, a more marked form of correlative function change, be so explained? If natural selection is to be ruled out as regards the elk's neck, it must more surely be ruled out as regards the giraffe.

I have already stated that Weismann, like a true Lamarckian, attributes variations to the influences of the environment on the germ-plasm during the ontogenetic development of the body. That being granted, we can readily perceive how change of habit can produce in time change of characters through inheritance of the functional modifications brought about through the change of habit. It is well known that many animals have, not one source of food supply, but several. A bird that visits a flower for honey may also be insectivorous. One source of food supply failing, the habit of constantly satisfying hunger from another is taken on; and this, by change in the method of feeding, leads to the increase of use of certain characters which co-operatively are brought into action, and the disuse of certain other characters. In this way distribution of animals or change of conditions *in situ* leads to new habits. But does the new habit modify the species in the direction of better adaptation to the new mode of life? I would answer that if the experiences of the mother influence the foetus, and act as external stimuli on the germinal cells, as is allowed by Weismann, we must see that changes in that experience, as brought about by a new habit, must be reflected on the foetus, producing a variation in the direction of better adaptation. And this process of better adaptation in each successive offspring must, in time, render the species fully adapted to its new mode of life. We find here not only the cause of variation, but the gradual process by which species through a change of habit becomes adapted to their new life. As the functional changes affect characters, new species are produced. Now, assuming that the Neo-Darwinians admit this *modus operandi* of the formation of new habits, our explanation of the inheritance of functional modifications of characters would harmonise the two schools, *i.e.* if we allow that Weismann represents the Neo-Darwinians.

Let us now consider how functional changes, as brought about by a change of habit, modify anatomically the characters affected. It must be plain that all modifications of form must have been

wrought by change in the environment, as otherwise heredity could never have any characters to work on. If that is not allowed, we must fall back on blind chance, or on the insinuation of some unknown power. Changes in environment can only be partial, since a complete change would destroy all organic life. But where changes are partial, and extending over vast periods of time, great changes may occur in the organism, as in the evolution of whales and seals from land animals. Now, where there are changes in environment leading to new habits in order to satisfy the vital functions, the organs or characters affected by the change of habit, being used in excess of their former use, are further developed, *i.e.* their cellular elements are increased, either absolutely or relatively or both, since increase of use means increase of nutrition. But the cell activity is brought about, not directly, but indirectly, through the connection with the nerve-centres. Hence the increase of exercise in the nerve-centre leads to increase of nutrition, and this in turn to increased development of the nerve-centre. Thus, with the increase of function, there is also increase in size of the characters affected, and of the brain centres presiding over them. Increased use of a muscle leads to increase in size, and the brain centre of the muscles must also be changed in some way, for it too has done increased work. We know that the memory may be strengthened by exercise, and so with other special mental faculties. So too, as regards the special senses, the sailor's eyesight is always better than the landsman's.

It is important, however, to remember that such changes take place chiefly in the young, and hence the importance of our contention that the condition of the maternal body—cells, tissues, and organs—affects the vitality of the developing ovum. The maternal conditions, acting as external stimuli to the ovum, must, as Weismann admits, affect the foetus, and I argue that they will produce such modifications as will bring the latter into harmony qualitatively and quantitatively with the maternal body. And as the general environment reacts on the mother, and the mother on the embryo, it must be evident that the general environment has some influence on the developing germ or embryo. Now as the general environment of a mother in her successive production of offspring must vary, so too must the offspring vary.

Assuming that the nervous system is to the fully-formed organism what the germ-plasm is to the ovum, we must see that there must be the same difference between the cells of the nervous system and the cells of the other portion of the organism as between germ-cells and somatic cells, for whereas the nervous system represents the whole body, a *multum in parvo*, and can induce the production of all kinds of cells, the somatic cells can only reproduce through the nervous system *cells of their own kind*. The egg-cell contains, as Naegeli says, all active specific characters as truly as the adult organism. What I

maintain is that each specific centre for a character in the germ-cell is represented in the nervous system by a specific centre for controlling such a character, *i.e.* that the specific centre in the germ-cell has developed into a specific nerve-centre in the central nervous system. It is only in this way that we can recognise the unity of the organism, and can understand the specific morphological characters of the organism.

In conclusion, I would call attention to a passage in Prof. Jordan's work, "Footnotes of Evolution," which expresses the position of Lamarckians as well as Neo-Darwinians. "The fitness by which organisms have been perpetuated is simply obedience or adaptation. Those which survive are fitted to the conditions of life. In other words, they are obedient to those conditions. Hence we may define the process as one of the survival of the obedient." Now whilst, as I have said, the above expresses well the conclusion of the Lamarckians as well as the Darwinians, the different standpoints of the two schools must not be overlooked. The Darwinian believes that obedience is at first restricted to the few in which favourable variations occur, and gradually through the production of more and more of such variations to the many; whilst the Lamarckian, recognising that the power to be obedient is a general law of nature, sees the obedient as the many, the disobedient being the few abnormal ones. Hence the main difference between the two schools resolves itself into this: The Lamarckian sees a general law of obedience, the Darwinians a law of opposition leading to a forced obedience.

If obedience is through natural selection operating on all characters, it is almost impossible to conceive that favourable variations as regards all characters can be present at the same time and in the same individual; if such should not be the case it must lead to the perpetuation of unfavourable variations as regards the unfavourable characters. Again, that the most favourable variations are weeded out through sexual intermingling is proved by this fact which is taking place constantly in all tropical countries. If the product of a black and a white person—a mulatto—with the favourable feature or character—the brown colour—intermarries with a white, and the descendants do the like, the favourable character—the brown colour—gradually disappears, until the descendants are indistinguishable from Europeans. Here the favourable character, which ought to have been preserved through natural selection, is gradually weeded out.

BARBADOS, W. INDIES,
July 1899.

The Fauna of the Sound.

Abstracted by F. A. Bather from the Swedish of Dr. Einar Lönnberg.

In two papers, entitled "Undersökningar nörande Öresunds djurlif," and "Fortsatta undersökningar," etc., and issued as *Meddelanden från Kongl-Landtbruksstyrelsen*, Nos. 43 and 49 (Upsala, 1898 and 1899), Dr. Einar Lönnberg has published the results of some researches made by him during June 1896, July 1897, and August and September 1898, under the auspices of the Swedish Office of Agriculture (Landtbruksstyrelsen). The language in which these papers are written, as well as the place of their publication, must prevent the majority of English readers from appreciating their considerable interest. The following attempt to present Dr. Lönnberg's general conclusions may therefore have some value.

Öresund is the narrow tract of water that divides Scania, the southern province of Sweden, from Själland, the island on which Copenhagen stands. Travellers from Denmark to Sweden cross its southern end as they go by steamer from Copenhagen to Malmö, while its northern opening is seen by the visitor to Elsinore. The Sound, as we usually call it, forms one of the connections between two sharply separated provinces of marine life—the brackish Baltic and the salt Kattegat. From the biological point of view it must be restricted within rather narrower limits than those usually assigned to it. Dr. Lönnberg draws the northern boundary from Hellebaek, a little north of Elsinore, to the projecting reef of Hittarp on the opposite Swedish coast. The southern boundary is marked by a broad bank stretching across by the islands of Saltholm and Amager, just south of Malmö and Copenhagen.

It is of course the case that the Sound, no less than the neighbouring seas, has been the subject of investigation by many naturalists. The Germans, for example, have their "Kommission zur wissenschaftlichen Untersuchung der Deutschen Meere in Kiel," together with the "Biologische Anstalt auf Helgoland"; Denmark has published "Det videnskablige Udbytte af Kanonbaaden 'Hauchs' Togter i de Danske Have inden for Skagen," and the reports of Dr. C. G. J. Petersen from "Den Danske biologiske Station"; while the Norwegian "Nordhavs-

expedition," and the writings of many other Scandinavian naturalists, trench more or less upon the region herein considered. Dr. Lönnberg also admits that his time and means have both been limited. He had only a little sailing-boat, with dredge and trawl no bigger than could be worked by hand. These facts add to the suggestiveness of his results. For, if he has been able, with such feeble opportunity, to add to the list, not merely of the Swedish marine fauna, but of forms new to science; if his work already enables him to foreshadow conclusions of scientific no less than practical interest, then it is clear that there is room for continued and still more detailed investigation. Considering the fluctuations in the number and kinds of fish that are said to have taken place in the Sound during this century, the mere list of captures has a certain value for comparison with past and future lists. Indeed the only previous list is that which Oersted published so long ago as 1844, in his little book "De regionibus marinis."

In a short introduction Dr. Lönnberg discusses the conditions governing the distribution of life in such a region as Öresund. The changes of wind and of current, which so frequently take place, may in a day or two completely alter the composition of the minute surface fauna, and thus induce a corresponding migration of such pelagic fish as herring and mackerel, which feed on these idly drifting organisms. To be of practical value, the study of such changes must continue from day to day. It is otherwise with the sedentary or slowly moving life of the bottom, and with the fish that feed thereon, such as cod and flat-fish. The constituents of this fauna, abiding in the same place from year to year, must be suited to the conditions there obtaining, and must be able to survive all those changes in salinity, temperature, and the like that may occur in the various seasons. Slow geological changes may have caused the fauna to alter slightly from its original composition, and may have eliminated some of its earlier elements; but their effect is more likely to be seen in a less favourable development of individuals. Experiment and observation have shown that many marine species can accommodate themselves to a slow reduction of salinity, or other change in the chemical composition of the water, although they may show signs of the change in their smaller size or less calcified skeletons. A difference of depth is not so important, and in any case since the so-called *Littorina*-age, which in the Baltic area was the immediate forerunner of present conditions, the amount of shallowing has not exceeded 5 metres. This, on the data generally accepted, and assuming a regularity in the change, implies a lessening in depth not more than 5 centimetres a century.

It follows from the arguments here briefly outlined that past fluctuations in, and the present distribution of, what one may call the edible fauna, with all their practical effect on the human neighbours, may be best interpreted by a detailed study of the present fauna,

and of the nature of the bottom, in which latter the varying character of the flora must be included.

Here we cannot reprint the annotated faunal list given by Dr. Lönnberg ; nor is this needed, since the universal language of systematic zoology will enable any one specially interested to learn from the papers themselves what species have been found.

The list of fish is complete, being supplemented from other sources than Dr. Lönnberg's own captures. It includes 98 species, of which 92 are purely marine. Of these latter, 41 are southern forms, stretching down to the Mediterranean, and never passing above the Arctic circle ; 30 are northern forms, stretching from the Arctic seas no farther south than the English Channel ; 11 have an intermediate or West-European distribution ; while 10 have a wider and less determined range. But when we consider the distribution of those fish that are permanent inhabitants of the Sound, or that appear there regularly year after year, the proportions are reversed. Such species number 47, and of them 14 are southern, 22 northern, 6 intermediate, and 5 wide-ranging. Comparison with neighbouring areas brings out several points of interest, of which a few may here be noted. The fish - fauna of Helgoland is less numerous (78 species), but as a whole the proportions of northern, southern, and intermediate forms are about the same as for Öresund. Among permanent inhabitants, however, Helgoland reckons a larger percentage of southern forms. The west Baltic has a fish-fauna of about 95 species. Many of these are fresh-water forms, of which only a few occur in the Sound. Of the salt-water forms almost all occur in the Sound, which also contains 21 species not found in the west Baltic. These latter, however, are more or less occasional visitors, and of them 10 are southern, 3 northern, 6 intermediate, and 2 wide-ranging.

In this fish-fauna the oldest stock consists of the northern species, which could live in these regions during or soon after the glacial period. Then, too, their range extended farther to the south, so that most of them reached the coast of France, and some got even as far as Spain. For others, however, such as *Drepanopsetta platessoides,* the southern limit was already reached in the Sound. A final class consists of pure relict forms, such as *Lumpenus lampetriformis* and *Cyclogaster liparis.* As the climate improved, species of southern origin could by degrees settle in the Sound.

The occasional visitors in the fish-fauna follow the various kinds of water in the marine currents ; thus the southern species come with the warmer and salter water in summer and autumn. When the conditions are altered by an influx of some other water, also when the temperature is lowered, many of these fish sicken and are thrown up on the beach, so that just before winter many southern fishes are found in this way. The southern immigrants are observed from June to December. The northern species that come with currents from the

north, are usually found from February till April. For all these fish the Sound forms, as it were, a large net with deep and wide intake towards the north, narrowing funnel-wise between Helsingborg and Elsinore, but widening again and deepening by Hven Island and Landskrona; but for a large part of the migrants the passage is completely closed by the sill-like shoal between Malmö and Saltholm.

Passing to the lower forms of animal life, Dr. Lönnberg mentions only such as he has himself observed, and gives careful notes on their habitats. The northerly nature of the fauna, already exemplified by the fish, is far more marked among these less wandering groups.

The Oscidians "have a distinctly Arctic stamp."

Among Mollusca, the bivalve fauna is almost entirely northern. Of 32 species, 14 are purely northern, while all the rest have been recorded from Arctic Norway. Of the 22 prosobranch gastropods, 9 are northern, 8 wide-ranging but chiefly northern, 2 wide-ranging but chiefly southern, although they are found at Lofoden as well as among glacial fossils; 3 alone are purely southern forms. Of the 4 shell-bearing opisthobranch gastropods, 1 is purely southern, but the 3 others, though having a southerly distribution, are found in Arctic regions. The 3 nudibranchs are all northern. Three of the chitons are purely northern; the 2 others wide-ranging, but do not reach farther north than Lofoden. In short, of all the 68 molluscan species, 42·66 per cent are purely northern; the same proportion stretches from the Mediterranean to the Arctic seas; 8·82 per cent find their northern limit at Lofoden; only 5·88 per cent are purely southern. A comparison of the measurements of 55 shell-bearing species from Öresund, the Kattegat, Arctic Norway, Kiel Bay, and the Mediterranean, gives the following results. The molluscs of the Sound are, as a rule, smaller than those of the Kattegat; those that are larger or of equal size are all Arctic forms. Compared with the molluscs of Arctic Norway, those of the Sound are smaller no more often than they are the larger, or of equal size. The molluscs of the Mediterranean are usually larger than those of the Sound, but the contrary is sometimes the case. The molluscs of the Kattegat are generally larger than those of Arctic Norway. Species that are common to the Kattegat and the Mediterranean are twice as often the larger in the Kattegat. This shows that salinity alone is not the effective factor in this case, but that other causes co-operate.

Among 16 species of the higher Crustacea, 7 are northern; 4 wide-ranging and reaching the Arctic; 3 are west European; and 2 purely southern.

Of the 41 or 44 species of Chaetopoda found by Dr. Lönnberg within the Sound as restricted by him, no less than 25 are purely northern; 12 are wide-ranging, but at least two-thirds of these have

been found off Greenland; only 2 or 3 are southern, and 2 intermediate. The northern character is even more manifest when one includes all species recorded in literature as found in the Sound, many of them, however, at its northern boundary. Fully half of the 48 are northern, and only one purely southern. The Chaetopod faunas of the Skagerack and Kattegat, on the other hand, contain more southern elements, and especially a large number of species with west European distribution—neither Arctic nor southern—a group that is but sparingly represented in the Sound.

The Bryozoa have not yet been thoroughly worked out; but of the 9 species found, as well as those previously recorded, *Membranipora membranacea* is the only purely southern form; the rest are either northern or wide-ranging, but for the most part found in Arctic seas.

The Echinoderma, of which there are 19 species, have a distinctly northern character. The 3 holothurians are northern. Five starfish are northern; the sixth, *Asterias hispida*, now first found in Swedish waters, is a Shetland form. Of the sea-urchins, 1 is northern and 2 wide-ranging. The brittle-stars comprise 3 northern forms, 2 wide-ranging, but tending more to the south, and 2 (alone among the Echinoderms) purely southern. There are in the Kattegat 18 more species of Echinoderma than in the Sound, and it is most suggestive that of these 8 are southern, 7 intermediate, 1 wide-ranging, and only 2 northern. Obviously the Echinoderm-fauna of the Kattegat is far more southern in its composition than is that of the Sound. So, too, among the 29 species of Echinoderma, known from Helgoland, only 9 are northern, the rest being wide-ranging or southern forms.

Only 16 species of Hydroidea have as yet been determined, but these add *Acaulis primarius* and *Cuspidella grandis* to the list of the Swedish fauna, while *Lovenella producta* and *Opercularella lacerata* have not before been found in Öresund. This part of the fauna has a northern character, more pronounced than that of Bohuslän, for example, from which, though it lies farther north, many of the northern species are absent.

The list contains notes on other zoological groups, but nothing of sufficient importance to be mentioned in this short abstract. The foregoing account is based on Dr. Lönnberg's first and larger paper; the second paper adds only three or four species, among which may be mentioned the new Hydroid, *Clava glomerata* (see *Zoologischer Anzeiger*, No. 578).

As already observed, the chief factor in the distribution of species within the Sound itself is the nature of the bottom. Dr. Lönnberg distinguishes the following regions and sub-regions: Shore-regions; Zostera-region; Alga-region, with Laminarian, Furcellarian, and Coralline sub-regions; deep-water, with bottom either of dead zostera, or mixed, or sand, or clay. Of course each of these divisions merges into those adjoining, but on the whole they may be characterised thus:—

1. Shore-region, reaches to a depth of 2 or 3 metres, with sandy bottom and a vegetation of Ulvaceae, *Fucus, Chorda,* a number of fine, thread-like green algae, *Potamogeton pectinatus,* and some zostera. For the list of characteristic species, reference must be made to the original paper. Most of them pass far up into the Baltic.

2. Zostera-region, usually with sandy bottom and zostera, from 3 to 15 metres deep.

3. Alga-region, broadly speaking from 15 to 20 metres deep, divided into (*a*) Laminaria sub-region, usually with a soft bottom of mud, often mixed with stones and shells; (*b*) Furcellaria sub-region, forming thick carpets with admixture of various red algae; (*c*) Coralline sub-region, with calcareous and red algae on a stone bottom. This last is more distinct in the nature of the bottom, and has a fauna more peculiar to itself, including many chitons.

4. Deeper water, without vegetation, usually outside the 20 metre line, subdivided thus: (*a*) dead zostera bottom, clayey or muddy with many dead leaves of zostera, which give it firmness and serve as food for several animals. This usually comes next to the Alga-region, and may reach a depth of 33 metres. When the zostera leaves are fewer and the clay mixed with sand, it passes over into (*b*) mixed bottom, which often contains many shells in a floor of sand and clay mingled in varying proportions; thus it passes into the two following: (*c*) sand bottom, often with shells, shell gravel, or shell sand; this is found in places where the current is strong enough to sweep away the finer mud, which goes to form the chief part of (*d*) clay or clay-mud; this, which is found in the greatest depths, is loose or oozy, but has no evil odour of decomposing organic substances.

In the Sound these various kinds of bottom do not, as in more open seas, succeed one another from shallower to deeper water, but depend rather on the currents, so that sand or mixed bottom may be found at greater depths than clay or mud.

From the facts given at length in the original papers it appears that almost every species of animal shows a preference for one particular kind of bottom. In many cases this is because they are suited to a certain mode of life, so that if, after the breeding period, the larvae sink on to a spot with unsuitable bottom or where other conditions of life obtain, the animals die off at once or in a short time. For instance, if mud from the depths be passed through a fine sieve, dead shells of young *Astarte* are often found, sometimes in great numbers. This shows that *Astarte* cannot exist on the soft mud. Its shell is too heavy: it sinks and perishes. On the other hand, its thick shell with stout epidermis is fitted to withstand rubbing and knocking against sand and pebbles, and a bottom of such nature is firm enough to prevent the shell from sinking into it. *Leda,* on the contrary, with its shell swollen up in front and beak-shaped behind, with its strong foot spread out like a sheet, is well equipped for living and boring in the

clay: so too is *Abra* with its thin light shell and long siphon. *Cyprina* also is prevented by its almost ball-shaped shell from sinking in the clay; at the same time it prefers a bottom mixed with sand. The long arms of the sandstar, *Amphiura*, and the felted spines on the under side of the heart-urchin, *Echinocardium*, must also bear up the animal's body on a loose bottom. *Natica*, which burrows with its outspread foot, has not much to fear from clay, though it usually prefers some other kind of floor. *Buccinum* thrives in clay: it is strong enough to work itself along there. *Cardium fasciatum* is found on all sorts of bottom. But the animals that do best in the clay mud are a number of Chaetopods. When, however, the clay is made firmer by admixture of sand, or by a carpet of dead zostera leaves, a far richer fauna is able to develop.

Difference of depth has here scarcely any effect on the distribution of species, since the whole Sound is so shallow that it would come within the littoral zone as usually understood. Such difference as there is has an indirect influence through its effect on the water. The southern sill and the narrowing between Saltholm and Scania cause the brackish currents from the Baltic to reach right to the bottom; but as the Sound widens again these currents broaden and thin out, so that their effects do not stretch so deep. Thus the bank between Malmö and Saltholm forms a complete barrier against the marine forms; the southern end of the Sound is occupied by a brackish water fauna, and the limit between this and the deeper salt water fauna gradually rises nearer the surface as it approaches the northern end of the Sound. The southerly increase of conditions unfavourable to a purely marine fauna differentiates the whole fauna into four classes according to the distance to which each penetrates the Sound.

We are now in a position to discuss the origin of the fauna of the Sound. We have seen how, in class after class, the species of purely Arctic or partly Arctic distribution outnumber those with a west European or more southern range. We have noted also that the proportion of northern forms is greater in the Sound than in neighbouring seas. Further than this, there are in the Sound a number of northern species which are not found in the Kattegat at all, or only in its most southerly portions, or which, if they do occur over the whole Kattegat, are not found in any quantity till one comes south. The Echinoderms furnish specially good examples. The holothurian, *Phyllophorus pellucidus*, is fairly common in the Sound, but only one specimen has ever been taken in the Kattegat, and that was in its southerly extension. It is not known off the more northerly Bohustän. But this species is typically Arctic; it occurs in the Norwegian Finmark and at Spitzbergen, and specimens found there cannot be distinguished from those dredged in the Sound. On the other hand, the *Phyllophorus* that occurs off western Norway, as well as the allied English form, both differ from that of the Sound. *Phyllophorus drummondi*, also taken in

the Sound, is another Arctic form that, on the coast of Norway, increases in number towards the north; it has been found in the Kattegat only at Samsö. Another holothurian, *Psolus phantapus*, does, it is true, occur in various parts of the Kattegat, but is more usual in its south-west corner, and is common in the mid-region of the Sound. The same is the case with *Cribrella* and *Solaster endeca*, although these star-fishes are not quite so common in the Sound. *Asterias muelleri* has only been observed a few times in the Kattegat, and then in its southern portions; but it is not rare in the Sound. *Crossaster* also increases in number towards the south. Again, a common brittle-star of the Sound, *Ophiopholis aculeata*, is rare in the Kattegat until its south-west portions are reached. In fact, as shown by C. G. J. Petersen, all the Arctic Echinoderms of the Kattegat are concentrated towards the south-west. Many similar examples are seen among the Mollusca, e.g. *Modiolaria nigra*, *Modiola* and *Bela trevelyana*. *Astarte borealis* is exceedingly rare in the Kattegat proper, and is also rare in southern Norway, but is common towards the Belt and in the Sound. *Chiton albus* is found only in the southern Kattegat, the Belt, and the Sound, *C. marmorens* begins to be common below Samsö, and so on. It would take too long to go through all the other classes of animals; one can just allude to such purely Arctic forms as *Lithodes* and *Mysis oculata*, which are found in the Sound, but not at all, or very rarely, in the intervening seas. It is clear enough that a large number of Arctic forms occur in the Sound (as also in the Belt) far removed from their natural area.

How is the existence and origin of this Arctic element to be explained? There are two possibilities. Either it has wandered in recently and is constantly recruited, or it has persisted here from a bygone age when conditions differed from those of to-day and were of an Arctic nature, like those which the forms in question now find in their proper home.

The first hypothesis seems at first to be supported by the existence of marine currents which every year, about February and March, bring water from Greenland to the Skagerack and the Kattegat. The fauna of the Sound and the southern Kattegat might therefore be recruited by larval forms floated across from Arctic regions in these currents. But to this view there are various objections. It is not likely that a larval form should float in the water long enough to complete the journey from Greenland to the southern Kattegat, since this occupies about half a year. The time required by the various forms to pass through their pelagic larval stages is not known for every case, but it can hardly be so long as half a year. Théel, for example, has shown that *Echinocyamus* needs no more than two months to develop from the egg into a sea-urchin crawling on the bottom. Mortensen has observed that masses of larvae of *Asterias rubens* and *Ophioglypha texturata*, floating in the Limfjord, remained there only a few days. The same author remarks that the floating larvae of Echinoderms are found chiefly near

the coasts, and do not belong to the true plankton of the high seas. The Hensen Plankton-expedition only once got as many as three Echinoderm-larvae at any distance from land. Only five species of Echinoderm-larvae were found out in the Atlantic, and three of these were in the Sargasso Sea.[1] Again, this first hypothesis does not explain why it is that these Arctic forms should be found in Öresund and not in other places, such as the northern Kattegat, where the opportunities for their development seem equally favourable. Moreover, many of the forms in question stretch north-eastwards along Finmark to Spitzbergen and the Kara Sea, but are not known from the coasts of Greenland: such are *Phyllophorus pellucidus*, *P. drummondi*, and *Asterias muelleri*. Some species of the Mollusca too are absent from Greenland, *e.g. Bela trevelyana*. But from Spitzbergen and the surrounding seas no current leads to the Kattegat. Then, too, if the first hypothesis were true, we should expect to find many other Greenland species, which, as it happens, are absent not only from the Sound but also from the Kattegat and Skagerack. Of twenty-nine species of Echinoderms found in Greenland, only eight occur in the Kattegat and the Sound. If some can cross, why not others? Take the case of *Cucumaria frondosa*, a holothurian common in Greenland waters, and with so wide a distribution that it stretches down America as far as Massachusetts, and down Europe from the North Cape to the English Channel. Yet it is absent from Bohustän, the Kattegat, the Sound, and Helgoland. This is a strong argument against the Greenland current theory. A still more forcible objection is furnished by the fact that some of the starfish in question (*Cribrella*, *Asterias muelleri*, *Crossaster papposus*), and perhaps other of the Echinoderms, have no pelagic larval stage at all.

It is clear that the first hypothesis fails us at many points. We have then to consider the second, and to inquire how long and wherefore these forms have remained in a district so isolated from the rest of their area of distribution. Two main groups of conditions determine the persistence of an animal in a given locality. One group includes the external chemical and physical conditions; the other, the relations of the organic world. The Sound, therefore, must afford conditions suited to the existence of Arctic animals, and at the same time less suited to the more southern forms with which they have to struggle. Arctic forms are accustomed to a low temperature, and also to great changes in the salinity of the water consequent on the melting of the ice.

[1] Two considerations seem to be overlooked by Dr. Lönnberg. First, the fact that a species *can* develop rapidly does not prove that it *must*. Experiment has shown that development may be greatly retarded by varying the conditions, and, for all we know, the necessary stimulus to complete development may be wanting in the current from Greenland so long as it is far from land. Secondly, as Alexander Agassiz, for one, has insisted, Echinoderms can be transported in other than the larval state; the young sea-urchin itself can be floated along. Especially is this the case when drift-wood or floating sea-weed comes to their aid.

Southern forms, on the contrary, enjoy a comparatively equable and high temperature and constantly salter water. Now, the water of the Sound is at all times of comparatively low salinity, and is, under the influence of winds and currents, liable to still greater reduction. Moreover, its shallowness, the influx of cold Baltic water, and the cold winds blowing from Sweden, combine to lower the temperature in winter almost to freezing-point to great depths, if not to the very bottom of the whole Sound. These conditions thus, while suited to the hardy northern species, are distinctly unfavourable to the more southern forms with which they contest the ground.

It is then intelligible that Arctic forms should continue to live in the Sound; but, since they have not entered recently and are not now coming in, they must have persisted there or thereabouts since a time when Arctic conditions were so widely extended that they embraced the now isolated Sound as well as the intervening areas. That took place during late glacial times. During the changes that succeeded, these Arctic forms must have changed their home and given way before the fresh-water streams from the *Ancylus*-sea;[1] but though many doubtless perished, a number of forms could brave it out, thanks to their power of resisting brackish water. When a fresh sinking of the bottom of the Sound let the salt water burst afresh into the Baltic, the Arctic forms came along with it by degrees, into the Sound and the Belt, and perhaps yet further; in this way they withdrew from the contest with the more southern forms that were now thronging up out in the Kattegat. This struggle with the more southern and more typically marine forms was then for a time even harder than now, since for a long period the water was much salter than at present, so that the oyster, *Tapes*, and other forms now extinct in those parts, could thrive there. It is therefore probable that it was just at that time—the *Littorina* period—that the break took place in the connection between the northern and principal area of distribution of the Arctic forms, and the more southern isolated districts, such as the Sound, where those forms still exist. After a time the Kattegat again became less salt, and a part of the southern marine forms (*Ostrea*, *Tapes*, etc.) died out. Thus began the existing state of things, in which the Arctic forms again found favourable conditions of existence, and possibly again extended their range.

Thus it is that, in the existence of an Arctic element, the fauna of the Sound presents a phenomenon like to that of Gullmarsfjord in Bohustän, and many Norwegian fjords, in which Arctic animals are found far south of their proper limit. Such persistent types are called relicts; and thus the fauna of the Sound may to a certain extent be called a relict fauna. The same term is perhaps also applicable to the fauna of the Belt. The conditions in these sounds are in a way like those in a fjord. In both cases is a narrow, enclosed water which com-

[1] Occupying more or less the district of the present Baltic.

municates with the sea at one end, and which is subject to a varying influx, in the one case, of fresh water, in the other of brackish water; in either case with the same result. Öresund especially is like a fjord, since the bank between Malmö and Saltholm forms a sill which prevents the deeper and salter layers of water from flowing right through into the Baltic. Those are the conditions that in great measure explain the composition of the fauna of the Sound.

The first paper ends with "some words on the vegetation of Öresund." Although the details are not full enough for any argument to be based on them, it is noteworthy that of the forms mentioned only two are lacking within the polar circle. At all events the facts corroborate the views above expressed regarding the origin of the fauna of the Sound.

The second paper presents a more detailed study of the extreme southern portion of Öresund. It contains many facts of scientific and practical interest, especially concerning the herring. But here we can only note that the general statements and explanations of the former paper are fully confirmed.

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Suggestions upon the Origin of the Australian Flora.

Continued from page 212.

By Spencer Moore, B.Sc., F.L.S.

Our scanty knowledge of the geology of the West Australian desert has recently been materially added to by Mr. Victor Streich,[1] who traversed the southern part of the desert lying between Mount Squires on the eastern border and Yilgarn on the west. Mr. Streich finds that Mesozoic rocks, covered in many places by abundant tertiary deposits, extend from Mount Squires as far west as Queen Victoria Springs, except in one place where Palaeozoic cliffs were seen. The rocks regarded as Mesozoic are clay, jasper-rock, conglomerates, and quartzite sandstone, and they are assigned to this age on lithological grounds alone, there being no fossils in them, but their lithological and stratigraphical features being the same as in the typical area outside the western colony. West of Queen Victoria Springs there are quartzite ridges, and at the Fraser Range hornblendic schists are met with. From the Fraser Range towards Lake Lefroy and the Hampton Plain, that is in the Coolgardie district, a series of metamorphic rocks are met with, the country having a general elevation of 1200 to 1500 feet above sea-level, while to the west lies an immense high plateau, 1300 to 1400 feet above the sea, terminating at the steep western escarpment of the Darling Range; there are several formations in this plateau, the granitic and the flanking schistose being the most conspicuous. The sandy flats covered with efflorescent salts on this plateau represent, Mr. Streich thinks, depressions of the granitic uplands in which has been accumulated the saline matter remaining over from isolated parts of the ocean. In the north-western part of this plateau the Crystalline hills are capped with desert sandstone, which directly overlies the granite and is invariably horizontally bedded. Fossils were not found in this sandstone, which Mr. Streich considers to be probably identical with the similarly named formation of Central Australia.

[1] "The Geology of the Elder Expedition," *Transactions of the Royal Society of South Australia*, vol. xvi.

The granites wherever they outcrop bear a most distinct eruptive character, elsewhere they are overlain by rocks of Palaeozoic or Archean age, composed chiefly of hornblendic schists and slates in different varieties, and themselves overlain by feldspathic schists and quartzites of the same age, with talcose and micaceous schists and siliceous ironstone.

I am unable to add anything of the least value concerning the northern part of the district visited by me, and which lies beyond the country traversed by Mr. Streich; indeed, a fair knowledge of British secondary and tertiary deposits is a most inadequate preparation for effective study of coeval formations in Australia whose lithological characters are so different from those of European deposits. I will merely remark that what, judging from Mr. Streich's description, appear to be secondary rocks are to be met with in the country between Mount Flora and Lake Darlot, although in the absence of fossils I must candidly confess I considered these formations to be much older. What I have specially in memory are sandstones and conglomerates; and the so-called "breakaways" of the country in question correspond apparently with the terraced outcrops of Mesozoic rocks Mr. Streich found in his eastern section.[1] But Mr. Streich's observations suffice to give us an idea of the changes undergone by the southern part of the West Australian desert since earlier Cretaceous times. We may infer from them a westward extension, probably in the form of a wide arm of the cretaceous sea which divided Australia into an eastern and a western island, while during earlier tertiary times the eastern part of the desert would seem to have shared the fate of Central Australia, that is to say, that after having emerged from the waves, submergence again took place while the tertiary formations were being deposited. Whether this part of Australia was subsequently a lacustrine area or whether it was dry land, does not appear from the evidence, though the presence of desert sandstone near Yilgarn suggests the former condition. The western part of the desert was above water during Mesozoic times, and if the Darling conglomerates be Palaeozoic, a considerable area west of what is now the desert was also dry land during these times. In earlier tertiary times the district must, in its eastern part, have borne the character of an archipelago, and by subsequent upheaval the sea was divided into a number of inland salt lakes which gradually underwent desiccation.

[1] Altitudes were taken during the course of this expedition. The country east from Queen Victoria Springs is from 1000 to 1200 feet above sea-level, thence it descends to the Springs (830 feet), and rises west of it to from 1200 to 1450 feet. The highest point of the Fraser Range is 2010 feet above the sea, and the plateau to the west of the range, as has been already mentioned, 1300 to 1400 feet, while Mount Monger near Coolgardie is 1700 feet above sea-level. A plan showing the elevation of the country between the Darling Range and Mount Burgess, the work of West Australian government surveyors, was issued about three years back in connection with the proposed Coolgardie water-scheme. It bears out, in the main, so much of the above statement as concerns the country in question.

It will be well here to refresh the reader's memory by giving a short *résumé* of the ideas enumerated with such acumen by Mr. Wallace. The fact that this celebrated naturalist's conclusions respecting the geological history of Australia are faulty, should in no wise render us blind to the immense ability revealed in his brilliant pages, one's only regret concerning which is that the requisite data were not to hand when the work was undertaken. Mr. Wallace holds that at one period, perhaps during the middle or latter part of the secondary epoch, Australia was connected with land lying to the north, whence it received the ancestors of its Monotremes and Marsupials. As he points out, for such a connection the general level of the country would have to be raised at least by 6000 feet, and this would change the whole country, including the deserts of the interior as well, into a mountainous and well-watered region, and in such a region the rich and peculiar flora characteristic of the south-west (the Autochthonian flora of Professor Tate) was evolved. While the western flora was in process of evolution, Eastern Australia, if it had arisen from the ocean, must have been widely separated from Western Australia, so that the present continent then consisted of a large and fertile Western Island, and a long and narrow island stretching from far south of Tasmania to New Guinea, with one or more large islands to the north. A depression afterwards occurred which buried the greater part of North Australia beneath the ocean; whence it emerged in the middle or latter part of the tertiary period, and was stocked with vegetation from South-West Australia on the one hand, and from Indo-Malaya on the other. The flora of Eastern Australia has been derived from three sources: its south temperate element coming from Antarctic lands, the tropical element of Polynesian types from the north or north-east, and the typical Australian from across the dividing strait. Thus Mr. Wallace accounts for the "mixed" flora of Eastern, the isolated flora of Western, and the intermediate flora of Northern Australia.

There are several objections to these views of Mr. Wallace. One, the existence in tertiary times of a sea separating the eastern and western part of the continent, has, as we have seen, no warrant from ascertained facts of geology, neither is there evidence for the submergence of Northern Australia on a wide scale at the period when Mr. Wallace supposes it to have taken place. Moreover, unless the Mesozoic upheaval was accompanied by much differential movement, the upraised area would be converted, not into a mountainous region, but into a raised plateau; while if mountain ranges of the supposed height were formed at all, their disappearance, with the exception of some insignificant hills, from hundreds of miles of country is wholly inconceivable. Besides, unless the geological record be extraordinarily defective, the date of the introduction of Marsupials is too early, seeing that remains of those animals are, with one exception in

Tasmania (Eocene), not met with earlier than the Pliocene age.[1] Mr. Wallace, it will be observed, adopts the conventional notions based on present distribution to which objection has already been made. On this view, if an Australian genus or species has the Indo-Malayan facies and is found outside Australia, or is closely related to extra-Australian forms, it must have migrated into its present habitat; but the palpable errors into which Mr. Wallace has been led while formulating what he believes to be the true explanation of the case, may perhaps lead us to suspect that there is something wrong in the inference from present distribution whereupon his views are founded.

Professor Tate's conclusions are also based upon notions as to present distribution. He considers the Australian flora to be composed of two elements, an endemic and an immigrant. The endemic flora is of three kinds: Euronotian in the south and east, Autochthonian in the south-west, and Eremian in the desert. The immigrant flora has two constituents—an Oriental, dominant in the littoral tracts, but mixed there with typical Australian genera, and an Andean, restricted for the most part to the highlands of New South Wales, Victoria and Tasmania, and with this he includes north temperate forms, that is species characteristic of north temperate regions. The Autochthonian element was dismembered in Cretaceous times, and except for possible inter-communication with the Euronotian *via* the present Eremian region during the period of tertiary submergence, and perhaps, too, by means of land in the south now submerged, it has remained in a state of isolation. The Euronotian element was modified during early tertiary times by the irruption of a primitive cosmopolitan flora. The Andean element was introduced during a glacial period, and since then the Eremian flora has been developed from Autochthonian and Euronotian constituents, largely modified by an incursion of Indian types, while at the same time the Euronotian gained accessions from the Indo-Malayan province, although migrants have probably been received at all times since the specialisation of the flora of the Indo-Malayan province.

It will be observed that Professor Tate is not content with making Australia a sort of botanical dumping ground during recent times, but that he ascribes a migrant character to the primitive tertiary flora as well. Is there sufficient justification for this? The primitive tertiary flora makes its appearance to all intents simultaneously in various parts of the earth, in North America, in Europe, at Perim, in Borneo, etc., as well as in Australia, and we have no evidence in any of these cases as to its origin in one of these localities, and of its migration into others. There seems also no conclusive evidence that the western part of Australia was absolutely isolated from the eastern half during earlier tertiary times, and it seems incredible, unless the climate of Western has greatly differed from that of Eastern Australia, that a flora which flourished over such a wide area as we have indicated, shall

[1] Tate, "Inaugural Address," p. 37.

have failed when it encountered a region which, except for some fresh-water lakes, interposed no bar to its advance. Moreover, Professor Tate's generalisation is the more unsatisfactory, inasmuch as we know nothing about the tertiary flora of Western Australia.

The idea that the primitive tertiary flora was an immigrant one, so far as concerns Australia, must therefore be regarded as exceedingly problematical. The wide distribution of that flora seems to show that, no doubt with local variations, all the countries inhabited by it enjoyed an approximately similar climate, and it is surely no extravagant hypothesis that Australia played a commensurate part with other countries in the evolution of the flora. Certain forms were of extra-Australian origin, doubtless; but we are not justified in assuming that one part of the great area peopled by that curious flora was shut out from the drama of evolution and condemned to be a passive recipient of forms generated elsewhere.

The key to the problem before us seems to be in the recognition of the fact of there being two main elements in the Australian flora, one xerophilous, the other hygrophilous, and by applying the same classification to fossil floras, and regarding the bulk of the forms having a typical Australian facies as xerophilous forms; the disappearance from countries outside Australia of natural orders and genera now confined to or characteristic of it can be accounted for without assuming the possession of some natural superiority by one flora over another. Let us take the case of Europe, which, during Miocene and still more during earlier tertiary times, had a climate considerably warmer than it has to-day. Now if, under these circumstances, the country were open and included stretches of desert (and this is precisely the character Mr. Wallace[1] considers it had during the Miocene age), here would be conditions exactly parallel in some parts of Australia, particularly Queensland, to-day. And what do we find there? In the drier parts typical Australian species flourish, while species of Indo-Malayan facies predominate elsewhere. It is therefore probable that the species of European tertiary floras referred to Australian genera were, for the most part, dwellers in the desert patches, while the moister places were occupied, to a large extent, by forms adapted to the conditions there obtaining. That the climate of Europe gradually changed during tertiary times we know, not only because the floras indicate decreasing warmth until the cold Pliocene age arrived, but because the great upheavals during the mountain-making epochs must undoubtedly have affected, in a marked degree, the near annual temperature of the upraised districts and of the countries in their neighbourhood. The diminution of its temperature would have the effect of rendering Europe better fitted to herbaceous vegetation; it would, in fact, change it from what I have previously called a dendritic to a herbaceous zone, and thus would be set up a tendency

[1] "Geog. Dict. of Animals," vol. i. p. 117.

towards the elimination of xerophilous forms. But it is not certain that the xerophilous vegetation completely disappeared from South Europe and Asia Minor, for it may well be—to cite a few instances only—that some Chenopodiaceae, and species of *Helichysum* (and a fair number of these still survive in countries bordering on the Mediterranean), may actually be descendants from herbaceous members of the xerophilous flora, and when the present distribution of these genera is borne in mind, there is, it is submitted, at least some probability for this view.

An objector will, of course, ask why it is, if the theory above sketched be true, that we do not now find species of *Eucalyptus* and *Banksia* and *Dryandsa* flourishing in deserts north of the equator. These, he will remark, are precisely the places to which a xerophilous flora would retire for shelter when driven by stress of climate from its former homes. Undoubtedly it would do so, if the desert then existed, and if no stretch of sea interposed to cut off the retreat of xerophilous species. The available desert country reaches from the Atlas Mountains across Arabia into Baluchistan, but from this the Sahara must be deducted, since it was, till quite recent times, submerged beneath the sea, and until the nummulitic limestone emerged from the waves, the ocean in which that extensive formation was laid down would be an effectual barrier to migration. Since Eocene times, however, this barrier has not existed; but it is not clear that Arabia and the drought-stricken regions bordering on it were deserts at the time when the two floras, xerophilous and hygrophilous, were engaged in their life-and-death struggle. If Perim can be taken as a guide—and there is no reason why it should not be—there is every reason to believe that Arabia enjoyed, in Eocene times, a climate much like that of Europe; and all we have to suppose is that the same change went on there as in Europe, namely that the climate became more favourable to hygrophilous forms, which were thus enabled to eliminate their xerophilous competitors, and that desert conditions subsequently prevailed, and the absence of Australian genera from the great northern deserts is explained. That this explanation presents difficulties is not to be denied, for the elimination, at least of arborescent forms, has been so complete, we should have expected that at least some few forms would have been able to adapt themselves to the altered conditions.[1] Still the disappearance of these forms is scarcely more remarkable than is the disappearance of "Northern" genera such as *Quercus* and *Alnus* and *Salix* from a country which must have afforded them, one would imagine, many eligible stations where they should have been able to survive.

Let us now turn to Australia. And first, one must express a

[1] The Indo-Malayan and East Asian species of the Proteaceous genus *Helicia* may perhaps be cases of adaptation in the sense used above; so, too, East Asian species of such genera as *Drimys*, *Baeckia*, *Leptospermum*, *Leucopogon*, etc.

doubt whether Professor Tate does not estimate too highly the rainfall of Central Australia during the *Diprotodon* period. The evidence relied on by Professor Tate is, it will be remembered, of two kinds, biological and physiographical. Now if the districts where *Diprotodon* remains are found were to a considerable extent lacustrine, conditions might well have prevailed there essentially similar to those occurring now in Central and South-Eastern Brazil, where, during the dry season, when the *cerrado* country is completely parched, a rich flora, maintained by condensation of vapour during the early morning hours, flourishes in the river-valleys. The remains, therefore, of large Herbivora by no means prove that the whole country afforded subsistence to those animals; and although the existence of lakes warrants the conclusion that the rainfall was greater than it is at present, there may still have been many places adapted to xerophilous vegetation, and to that alone. As regards the physiographical evidence, we have to take into calculation the probable effects of a great lowering of temperature, even if the agency of ice cannot be invoked. Professor Tate and Mr. Watt,[1] after a careful examination on the spot, deny the evidence of ice-action, although to explain certain phenomena easily susceptible of such an explanation, they are driven to resort to a theory they themselves admit is "wild in the extreme." On the other hand, Professor Baldwin Spencer and Mr. T. M. Byrne,[2] as the result of a recent investigation, have no doubt of ice-action in Central Australia, though they decline to say at what period it was in operation. But apart from this, we have incontestable evidence for a cold climate in South Central Australia during past Miocene times, when the southern part of South Australia, as is well shown at Hallett's Cove, was glaciated. Now, under these circumstances, much of the precipitation falling on Central Australia would take the form of snow, which, during the summer months, would melt and thus release large bodies of water sufficient to cause a considerable amount of denudation. On the whole, therefore, we need not suppose that the rainfall of Central Australia during the period under review, although doubtless greater than at present, was so excessive as to prevent xerophilous vegetation flourishing side by side with hygrophilous, and contrasting Central Australia with Europe there is reason for supposing that while in the latter case conditions favouring a mixed xerophilous and hygrophilous flora were gradually changed in the interest of the hygrophilous element, in Australia the converse held, the tendency in favour of xerophilous forms continuing into the present day.

What was the climate of South-Western Australia during early tertiary times? Did the primitive tertiary flora flourish there as in other parts of the world? There is no reason to doubt that it did,

[1] "Report of the Work of the Horn Scientific Expedition," p. 70.

[2] Reported in *Nature*, vol. lvii. p. 495 (1898). A supposed Queensland case is also alluded to here.

provided the climate was suitable to a mixed flora. Unfortunately this is precisely the question to which no answer is possible in the present state of our knowledge, and it is greatly to be wished that steps should be taken to examine the lignite beds of the Fitzgerald River,[1] which would in all probability suffice to solve the problem. But whether the primitive flora in its entirety flourished there or not, it is submitted that the peculiar flora of the south-west can be explained on the assumption of a great difference in climate between the south-west and other parts of Australia, a difference dating either from Eocene (possibly late Cretaceous) times or from some period shortly after the Eocene. We have to suppose that long before the great plains of the central and eastern interior became desiccated, a considerable area in the south-west was already under the influence of drought. Westward of this dry district, which could have supported only a meagre flora, a somewhat more genial climate must have prevailed, but one favourable, except in isolated areas, to xerophilous vegetation. And if any difficulty is felt in adopting this view on account of the proximity of the ocean, it must not be forgotten that in the Shark Bay district desert conditions actually prevail up to the sea-coast, and moreover that, although southward of this district there is a fair annual rainfall near the ocean, the precipitation rapidly diminishes in amount at short distances from it. I saw many scenes of desolation in the interior, but they were almost equalled in the neighbourhood of York, barely sixty miles inland, in the early summer of 1894; even on the coast itself, periods of drought, during which non-xerophilous vegetation has but a slender chance of survival except in specially favoured localities, are frequent in the summer season. In such a country as this, then, I venture to believe the rich flora of the south-west to have been mainly evolved, and not, as Mr. Wallace supposes, in one diversified by lofty mountain systems, which, if they ever existed, have, except for the lowly Darling and Stirling Ranges, the upland districts of the far North-West, and the insignificant hills scattered over the vast intervening territory, vanished without leaving a trace of their former presence.

I cannot agree with Professor Tate in thinking that, except very rarely, there has been no interchange between the floras of Eastern and Western Australia. The flora of the Western desert has a fair number of species common to the two areas; there are also a considerable number common to the desert and the south-west, and some eastern species which advance to a greater or less distance into the desert, but without reaching the western coast region, and all this seems to indicate a filtration, most probably very slow, across the desert plains. It may also be remarked that recent discoveries have diminished the number of large genera having no species common to

[1] This and a thorough exploration of the North-Western territories are the two most interesting and important achievements now remaining to naturalists in Australia.

the two areas. But the evidence from species is not a satisfactory disproof of communication, for desert varieties are, as is well known, rather frequent, and if in the course of ages species have been differentiated from such varieties, a fair amount of concealed interchange may have taken place. The evidence is much stronger in respect of genera restricted either to the east or the west side of the country. Of these there are a large number which have not succeeded in making the passage, although many—xerophilous ones especially—have advanced some way towards doing so. These cases give emphasis to the conclusion that interchange across the desert has taken place very slowly, and to no considerable extent on the whole, although I cannot help thinking that Professor Tate decidedly underestimates its amount.

For long periods, perhaps since Cretaceous times, the evolution of the flora of Eastern and Western Australia has proceeded along different lines. So far I am in accord with other writers, and indeed this seems the only inference to be drawn from the facts. But the main reason for this is to be sought, as I venture to think, not in the simple isolation of the western part of the country while the eastern has been accessible to migrants from outside which have made headway against the endemic vegetation in consequence of their inherent superiority, but in climatic difference which had already become pronounced while the eastern interior was still a comparatively well-watered country. In short, I see Western Australia to-day supporting a vegetation similar, in its chief elements, to that which would now have been flourishing in Europe if our continent had been undergoing desiccation since Miocene times, and without lowering of its mean annual temperature. The interposition between the two halves of Australia of a sea and of a desert has, no doubt, laid an embargo on migration from one to the other. But for these barriers many restrictedly eastern forms would now be found upon the western seaboard, and *vice versa*. But we are not warranted in supposing that interchange would have taken place to such an extent as to result in a homogeneous flora; for the areas in Western Australia suited to hygrophilous forms are strictly limited, and the preponderating xerophilous element in the western flora is so well adapted to the extraordinary conditions prevailing in the west as to render its displacement in the highest degree unlikely.

Turning now to Eastern Australia, we find there a flora with little ordinal difference from that of the west, but containing many genera and a large number of species which, if they advance westward into the desert at all, do not reach the coast. Moreover, speaking generally, as we proceed northwards, and this applies generally to the moister regions near the coast, the number of forms possessed in common with Indo-Malaya and of forms allied to such tends to increase. There is also a sprinkling of species now characteristic of northern

lands and of species congeneric with these. A few of these species occur also in Western Australia; in the eastern districts they are more abundant in the cooler mundane country, and it is especially in the latter that are found forms conspecific with, or closely allied to, forms now forming part of the Antarctic flora, accepting this term in the conventional sense, that is, as embracing genera now largely or entirely restricted to southern cold temperate lands. It is admitted that these facts do, at first sight, favour the view of migration on a large scale followed by partial overpowering of the indigenous flora. But when we consider what is known of the history of Australia since late Cretaceous times the matter wears a different aspect. We have every reason to believe that since these times considerable portions of Eastern Australia have enjoyed a climate almost identical with that of Indo-Malaya, a climate, too, still prevailing in the north and north-east. We know, moreover, that in early tertiary times the floras of both countries were in a large measure identical. Is there anything remarkable, therefore, in the evidences of floristic affinity between the two regions? It will perhaps be conceded, as Professor Tate himself has conceded, that a certain proportion of the Australian species of genera common to these two neighbouring areas are descendants from the primitive flora, but that by far the larger number are immigrants. This, however, assumes our possession of complete records respecting the two floras from Eocene times to the present; and that we have anything like such records is an assertion no competent person would take upon himself to maintain.

But migration there has been, and the number of identical species and such a fact as the discovery of outlying forms allied to Indo-Malayan on the Bellenden-Ker range in North Queensland prove it, as also does the existence of "Australian" forms on the mountains of New Guinea, and in less number in various parts of Indo-Malaya and Eastern Asia, if, indeed, these last be not descendants from the primitive flora. Moreover, the trend of migration from the north has undoubtedly predominated over that from the south. But are we justified in assuming from this that any superiority is inherent in the Indo-Malayan flora over the Australian? What are the data? A number of hygrophilous genera and a certain proportion of hygrophilous species are common to the two regions. Now the Indo-Malayan flora, exception made for that of Timor, in some measure xerophilous, is and has long been a hygrophilous flora; while in Australia since Eocene times, if the view above enunciated be correct, hygrophilous types have had to struggle with xerophilous ones, which latter to-day still form a large element in its flora. Whatever in this case the means whereby migration has been brought about, its trend must, other things being equal, have borne direct relation to the size—or what comes approximately to the same thing, the comparative floristic richness—of the areas between which the interchange has been made. We have

also only to consider the hygrophilous element in the two floras, since the Indo-Malayan climate is not suited to xerophilous ones. While, therefore, the considerable areas in Northern and North-Eastern Australia[1] favourable to hygrophilous species have been open to the incursions of the whole of the rich Indo-Malayan flora, only those Australian forms adapted to hygrophilous have had a chance of penetrating into Indo-Malaya. It is submitted, therefore, that a preponderant migration from the north is only what ought to be expected on the doctrine of chances, and that there is no need to import into the discussion notions as to relative superiority and inferiority. We thus stand here upon precisely the same ground as that taken up in considering the supposed aggressive power of the Scandinavian flora.

The case is different with the Antarctic element of the Australian flora. This comprises forms suited to the lower grades of temperature, and all available evidence teaches us that colder conditions have been, of course in a geological sense, temporary only in Australia. But bearing in mind that glacial effects must have lasted a very long time, as contrasted with the span of human life, we may suppose that species of which the ancestors were received from the south may have been differentiated within the wide area in Australia suited to Antarctic forms during glacial times and times immediately preceding and following them, and that some at least of such species, accompanied by native ones which had become adapted to colder conditions, would migrate south when glacial conditions passed away, and so add a new, if small, element of Australian origin to the Antarctic flora. In any event, the Antarctic element seems to be an immigrant one.[2] I do not remember any attempt to prove from the presence of Antarctic forms the possession of "aggressive power" by the Antarctic flora, though, as the evidence for migration is so much stronger in this case, the omission, to say the least of it, is somewhat strange.

But Professor Tate tells us that a flora of exotic origin is in the act of displacing its indigenous vegetation from Central Australia. Let us see upon what evidence this conclusion reposes. Most of the truly Australian forms, he says,[3] usually grow gregariously or in isolated colonies from a few square yards to several square miles in area. But in a country like Australia, where good patches of soil alternate with bad ones, this gregarious habit scarcely implies want of adaptation. I saw precisely the same thing in Western Australia, and the inference I drew from it was directly contrary to Professor Tate's, namely, that the large numbers of a species monopolising or almost monopolising considerable portions of ground argued success in their

[1] And to a somewhat more limited extent the North-West too.

[2] Some of the herbaceous genera now characteristic of northern lands represented in Australia may have been introduced from the south during the glacial period.

[3] "Report of the Horn Expedition" (Botany), p. 120.

competition with other forms. Further, Professor Tate thinks the aggressive nature of alien plants to be exhibited not only by their extensive distribution, but by their ability to adapt themselves to extremes of soil and climate. He cites the following species in illustration: *Tribulus terrestris, Cleome viscosa, Malvastrum spicatum, Boerhaavia diffusa, Salsola kali, Mollugo hirta,* and *Pollichia zeylanica.* Now, even granting these to be aliens, and I think there are grave reasons for doubting the exotism of more than one of them, if these alien migrants from a hygrophilous zone are better adapted to desert conditions than native species which have had the advantage of long adaptation, it is strange that their distribution in the desert should be so restricted. Only four of the seven have been recorded from the western desert at all. I myself met with but two of them, viz. *Tribulus terrestris* once only, and *Salsola kali* about half-a-dozen times, but on only one occasion in any quantity.[1] Moreover, it should be remembered that these are all herbs of wide extra-Australian distribution, and provided, most of them, with special means of diffusion. Their presence in the desert is, therefore, easily explicable, and there is no warrant for drawing, as an upholder of current notions might wish to draw, from the fact any inference as to the superiority of an exotic flora over the native flora as a whole. On the gorges of the tablelands and on the basal part of the craggy escarpments and their taluses Professor Tate found a mixed flora in which the endemic element predominated, ten per cent only of it being of (supposed) exotic origin. The exotic species are seven in number; of these, except *Hybanthus enneaspirmus,* reported only from Mount Squires, on the eastern border of the western colony, *Parietaria debilis* alone has been found in the western desert. And when we compare the two lists above-mentioned, a curious fact comes out, namely, *that the name of not one species occurs in both,* and this forces one to suspect that Professor Tate has overestimated the adaptability of these supposed alien species. It would be wise, therefore, to reserve judgment on so difficult a point as that mooted by Professor Tate.

The view here taken up, it will be observed, is one intermediate between that of writers who, basing their conclusions on present distribution alone, profess to trace "currents" of vegetation from one part of the world to another, and ascribe the moving force, if the term may be allowed, of these currents to some natural inferiority of forms native to the country towards which the current is supposed to set—between this view and that of Baron von Ettingshausen,[2] who, while

[1] I should have been only too happy to come across *Salsola kali* more frequently, as it is an excellent fodder for camels. Doubtless it is much more common in South Australia, for on the occasion referred to above, our Afghan, who had worked at camel establishments in the eastern colony, at once recognised the plant, calling it "South Australian salt-bush," and informing me that it is an important fodder-plant there.

[2] "Contributions to the Tertiary Flora of Australia," *Mem. Geol. Surv., N. S. Wales,* 1888.

maintaining that the evolution of the existing floras from the tertiary flora was effected through differentiation of climate—and this, I venture to think, has been a main cause of floral diversity—dismisses as "absurd" the doctrine that certain floristic identities and affinities between regions now separated by the ocean are to be explained on the hypothesis of a former land-connection between them. I believe such land-connections to have existed, and, indeed, the present distribution of animals vouches for the truth of the theory. But until we know a great deal more than is at present known about the floras of any two countries previous to their being placed in continuity, I fail to see the possibility of estimating, except as mere guess-work, the respective effects upon the two floras so connected. Further, I believe the inferences from present distribution and from floristic superiority and inferiority upon which the current notions as to the origin of the Australian flora are founded, to be in the highest degree misleading. So facile a solution of the problem may commend itself to some; but it may be that many an observer will have to add his contributions of fact and suggestion before the final solution is reached; and if the notions here propounded, though they should fail to find acceptance, should at least drive home the conviction that much yet remains to be done in this fascinating field of research, the writer's object will have been fully attained.

FRESH FACTS.

INFLUENCE OF COLD ON DEVELOPMENT. OSKAR SCHULTZE. "Ueber die Einwirkung niederer Temperatur auf die Entwickelung des Frosches," Zweite Mitteilung, *Anat. Anzeig.* xvi. 1899, pp. 144-152. Prof. Schultze published a communication on this subject in 1895, in which he stated that subjection to zero temperature brought the development of the eggs of *Rana fusca* to a standstill. Further experiments have, however, convinced him that this is not the case. Even at zero the cell-divisions continue, though more slowly. He has not been able to bring about a complete non-fatal standstill in the frog's development; if it is producible, it must be by temperature below zero.

A SNOW-WORM. J. PERCY MOORE. "A snow-inhabiting Enchytraeid (*Mesenchytraeus solifugus* Emery) collected by Mr. Henry G. Bryant on the Malaspina Glacier, Alaska," *Proc. Acad. Nat. Sci. Philadelphia*, 1899, pp. 125-144, 1 pl. A somewhat detailed account is given of the structure and habits of this worm, which has so remarkable a home. A very striking peculiarity is the yellow-brown, deep chocolate-brown, or almost black colour, and its opacity. Associated with it was a small Podurid, *Achorutes nivicola*, also black, and there are other instances. "It seems probable that some factor in a snowy environment lays the brand of melanism upon all the constituents of its invertebrate fauna." But "zoological literature fairly bristles with attempted explanations of melanism." The author discusses the physiological interest of an animal which lives and grows while maintaining a body temperature seldom varying much from the freezing-point of water.

FACTS OF INHERITANCE. ERNEST WARREN. "An observation on inheritance in parthenogenesis," *Proc. Roy. Soc.* lxv. 1899, pp. 154-158, 1 fig. Dr. Warren has made measurements of successive generations of *Daphnia magna*, which, though insufficient in number, "appear to favour the view that inheritance in parthenogenetic generations resembles that from mid-grandparent to grandchildren." "If this kind of inheritance be found to hold at all generally in parthenogenesis, it would be a fact of very considerable significance, and might conceivably give some insight into the physiological causes of heredity and variation."

HAS THE HAG A PARIETAL EYE? F. K. STUDNIČKA. "Zur Kritik einiger Angaben über die Existenz eines Parietalauges bei Myxine glutinosa," *SB. böhmisch. Ges. Wiss.* 1898 (published 1899), 4 pp. In one specimen of *Myxine*, Dr. Beard observed in 1889 a distinct parietal eye, but Retzius, Saunders, and Leydig sought for it in vain. More recently, Studnička has joined in the search, and is emphatic in declaring that there is no trace of the organ to be found.

THYROID AND THYMUS OF AMPHIBIANS. HERMANN BOLAU. "Glandula thyreoidea und Glandula thymus der Amphibien," *Zool. Jahrb.* xii. 1899, pp. 657-710, 11 figs. Two kinds of thyroid occur, one with colloid vesicles, the other with a connective tissue meshwork including leucocytes and blood-vessels. The number on each side differs in nearly related forms, but there is never more than one colloid gland on each side. In Ecaudata the gland is always colloid except in *Molge rusconii*. The thymus is single on each side in Ecaudata and Caudata, except in the larval form of *Amblystoma tigrinum*, which has a variable number. In *Siphonops*, as Leydig has shown, there are

four in a row. The content of the thymus is a connective tissue meshwork with included leucocytes, besides Hassall's corpuscles, and sometimes fine capillaries.

THE SAURIAN DIAPHRAGM. F. HOCHSTETTER. "Ueber partielle und totale Scheidewandbildung zwischen Pleurahöhle und Peritonealhöhle bei einigen Sauriern," *Morph. Jahrb.* xxvii. 1899, pp. 263-298, 1 pl. and 6 figs. The question has often been raised whether the mammalian diaphragm—the diaphragma dorsale—is a distinct and independent structure. Goette and others have contributed to the answer. In the paper before us Herr Hochstetter shows from studies of *Stellio vulgaris*, *Lacerta agilis*, and other lizards, that there are Anlagen present which furnish adequate basis for the evolution of the mammalian structure, and also for the very different diaphragm of embryo-birds.

GREEN PIGMENTS. MARION I. NEWBIGIN. "On the affinities of the enterochromes," *Zool. Anzeig.* xxii. 1899, pp. 325-328. While acid acts on an alcoholic extract of green leaves in such a way as to produce the pigment phyllocyanin, which is insoluble in alcohol and ether, its action on an alcoholic extract of green Algae results in the production of a pigment which is exceedingly soluble in alcohol, and does not therefore precipitate from acidified alcoholic solutions unless a considerable amount of water be added. In its colour and fluorescence, in its spectrum, in its changes in colour and spectrum on the addition of acid, in its solubilities, the pigment shows a remarkable resemblance to the enterochromes. This resemblance is such that, taken in conjunction with the recent observations and conclusions of Dr. M'Munn in the case of "enterochlorophyll," and with the fact that that pigment occurs in the faeces of *Patella*, it seems to justify the conclusion that "enterochlorophyll" at least is an acid derivative of chlorophyll, produced by the action of the digestive juices on the chlorophyll of the food. Whether the other enterochromes, and notably chaetopterin, are produced in the same way, cannot as yet be determined. There can, however, be no doubt that the enterochromes are at least closely related to the pigment produced by the action of acid on the chlorophyll of green Algae.

WANDERINGS OF WARBLES. P. KOOREVAAR. "The larval stage of *Hypoderma bovis*," *Ann. Nat. Hist.* iv. 1899, pp. 69-73. Translated by E. E. Austen from *Tijdschrift der Nederlandsche Dierkundige Ver.* v. 1898, pp. 29-34. Various experiments intended to throw light on the obscure corners of this life-history have led Mr. Koorevaar to the opinion that the young larvae of *Hypoderma bovis* at first pass beneath the skin; and thence betake themselves to the spinal canal and other places, to return later into the subcutis, and there undergo further development under the well-known conditions.

LUMINOUS ORGANS. LEOPOLD JOHANN. "Ueber eigenthümliche epitheliale Gebilde (Leuchtorgane) bei Spinax niger," *Zeitschr. wiss. Zool.* lxvi. 1899, pp. 136-160, 2 pls. and 1 fig. Brown or black spots on the skin of this fish turn out to be luminous organs. Their origin is like that of skin-glands; their elements are differentiated as luminous cells and lens-cells; their luminosity was observed by Dr. Th. Beer.

A STRANGE CREATURE BROUGHT TO LIGHT. CHARLES MINOR BLACKFORD. "A Curious Salamander," *Nature*, lx. 1899, pp. 389-390, 2 figs. [Letter]. From an artesian well, sunk 188 feet in limestone, near San Marcos in Texas, various white and blind crustaceans have been obtained. Even more striking, however, is a salamander, believed to represent a new genus and species. It has been named *Typhlomolge rathbuni.* It is from 3 to $4\frac{1}{2}$ inches in length, and dingy white in colour, except on the external gills where the red blood shines through. The eyes are completely covered by the skin, but are seen from the outside as two black specks.

SOME NEW BOOKS.

PATHOLOGY OF PLANTS.

A Text-Book of Plant Diseases caused by Cryptogamic Parasites. By GEORGE MASSEE, F.L.S., Principal Assistant (Cryptogams), Royal Herbarium, Kew. Crown 8vo, pp. xii. + 460. London: Duckworth and Co., 1899. Price 5s.

THE announcement of a "Text-book of Plant Diseases caused by Cryptogamic Parasites," written by Mr. George Massee of the Kew Herbarium, naturally raised anticipations that the book would be distinctly better than any existing English work on the subject. The book now before us, on the whole, justifies the expectation, and is a welcome addition. The author's stated object is to give the inquiring grower of plants information on diseases caused by fungi and allied organisms. The introductory fifty pages give a general summary on the principles of preventive treatment, and the preparation and application of remedies. The greater part of the book (pp. 53–349) is occupied by short descriptions of the more important diseases. These descriptions are brief, yet clear, and free from unnecessary detail; they are well illustrated with figures mostly original, and the means of prevention are given in each case. The accuracy of this part of the work is guaranteed by Mr. Massee's long experience. The arrangement of the descriptions in the order of the fungi causing them is perhaps that least convenient to practical growers. The diseases described are selected—in a book of 450 pages this is essential—but the method of selection is vague. Only cultivated plants are considered, yet one sees no mention of several familiar British garden diseases, *e.g.* the smut and the rust on violets. Such omissions, and the complete neglect of the diseases of wild plants, many of which may easily attack cultivated forms, render the ambitious title of "Text-book" misleading. The inclusion of lengthy descriptions of diseases of exotic plants (*e.g.* tea and coffee) seems out of place in so small a book; it is doubtful whether the description of a few diseases will greatly assist growers in our colonies, while it introduces a confusion as to whether the disease under consideration is important in Britain, especially since this is not always clearly stated. On p. 349 the reader will find himself suddenly introduced into a maze of 100 pages of terminology peculiar to a fungus-flora. Reference to the "Contents" explains that these are "scientific descriptions of the fungi enumerated as causing diseases." Are these intelligible to many growers of plants? Are they, considering their limited number, of much use to the scientific worker? We venture to believe that an extension of the earlier parts of the book, and the omission of this portion, would have made it look more appetising to the practical man. Such defects can, however, be remedied, as also may some minor faults in typing and reproduction of drawings. The book is the work of the best investigator on the subject in Britain, it contains much valuable information in a readable form, its price is moderate; hence it is an indispensable addition to the library of the plant-grower who would learn as much about his subject as he can. W. G. S. (LEEDS).

FERMENTATION.

The Soluble Ferments and Fermentation. By J. REYNOLDS GREEN, Sc.D., F.R.S., Professor of Botany to the Pharmaceutical Society of Great Britain. Pp. xiii. + 480. Cambridge University Press: C. J. Clay and Sons, 1899. Price 12s.

We can thoroughly recommend Prof. Reynolds Green's book to all who wish to obtain a trustworthy guide to a correct knowledge of the processes of fermentation. When we consider that it is little more than half a century since Pasteur discovered that the cause of alcoholic fermentation is the activity of the yeast plant, the strides made in our knowledge appear prodigious. There is hardly any aspect of animal or vegetable life in which ferments do not play some part. The ferments that cause our food to be digested, that produce clotting of the blood, that bring about oxidation, that minister to plant life in various ways, that are associated with the putrefactive and other changes wrought by bacteria, are all described with full details, and in a lucid, interesting manner; the history of the subject is also well given. The distinction between the organised ferments, like yeast and bacteria, and the soluble or unorganised ferments or enzymes, such as pepsin of the gastric juice, and diastase in germinating seeds, is a useful one. But the difference is more apparent than real, for probably in all cases the micro-organisms which are called organised ferments perform their work by secreting soluble ferments or enzymes. The recent work of Buchner has certainly shown this to be the case for yeast. What enzymes really are, and how they produce changes in large masses of material without any apparent change in themselves, or in their power, are much more difficult points to answer; the reader will, however, find in this book such general questions discussed in the light of recent knowledge. The ferments themselves, so far as any positive statement can be made, appear to be real chemical substances, and in their composition are allied to the albuminous bodies, particularly to the class known as nucleo-proteids. The interesting recent work of Emil Fischer, which is very clearly described, shows a possible way in which such substances could produce the change known as fermentation. All these theories are, however, tentative; whether they will stand the test of time, the future only can show. H.

A MODEL FLORA.

The Flora of Cheshire. By the late LORD DE TABLEY (Hon. J. BYRNE LEICESTER WARREN, M.A.) Edited by SPENCER MOORE. With a Biographical Notice of the Author by SIR MOUNTSTUART GRANT DUFF. Pp. cxiv. + 399, with a Portrait of the Author, and a Map of the County. London: Longmans, Green, and Co., 1899. Price 10s. 6d.

Cheshire botanists will welcome the appearance of this flora of their county, though an interval of close on quarter of a century has elapsed since the preparation of the materials now for the first time printed. During such a period many changes must of necessity have taken place in details of local distribution, but the flora has been brought as far as possible up to date by the editor, and is a model of what a county flora ought to be.

The author has devoted great attention to describing, often at considerable length, the physical features, soils, etc., of the specific habitats, a point of great biological importance, and one too often overlooked. Duly authenticated, and, where possible, personal records are provided of the occurrence of each species in each of the seven hundreds, and a striking feature of the book is the care expended on the enumeration and history of alien and introduced species, many of which are traced back to their origin in ballast heap or garden, while numbers

of mere casuals are mentioned either as having been personally observed or recorded in the past. The flora proper is accompanied by an all too short physico-botanical account of Wirral (unfortunately the only hundred so treated), a short account of the Bucklow hundred, and a bibliography of Cheshire botany. J. A. Terras.

TELEOLOGY.

Elemente der empirischen Teleologie. By Paul Nikolaus Cossmann. 8vo, 132 pp. Stuttgart: A. Zimmer's Verlag (Ernst Mohrmann), 1899. Price 4 marks.

There are some biologists who think, or who speak as if they thought, that teleology is a vestigial organ in culture—a way of looking at things which has had its day, and must gradually cease to be. Purposive structure and function —adaptation in short—they admit, but Darwinism has supplied the "mechanical explanation," and teleology is an irrelevancy. To others it seems that in biology we have not yet got very far in discovering the causal chains, the last link of which is an adaptation, and that even if we had got much further, we should have reached only a formulation in simpler terms. To these, teleology appears no irrelevancy, but a necessity of thought. Far from destroying teleology, Darwinism has rather deepened it.

The author of this book is an ardent teleologist, who seeks to show how difficult it is for us even in our scientific phraseology to get away from teleological conceptions, and how partial the outlook is which rests satisfied with chains of cause and effect. In working out these, the teleological idea is irrelevant and even inhibitive; for their development as parts of an intellectual system it is, however, necessary, since so-called scientific explanations are not explanations at all. The book is full of quotations and illustrations intended to show the difficulty of eliminating teleological conceptions from biology, and the utility of appreciating them. It might be described as a plea for a franker recognition of the purposive, and should be interesting to students of "Methoden lehre" and the philosophy of biology. X.

A FALSE ANALOGY?

La spécificité cellulaire, ses conséquences en biologie générale. By L. Bard. Professeur à la Faculté de médecine de Lyon. (Scientia. No. I.) 100 pp. Paris: Georges Carré et C. Naud, 1899. Price 2 francs.

In the young organism, or young organ, there is often apparent uniformity among the component cells. As observed by our methods, they show no hint of the variety of cellular type which will gradually arise among their descendants. Many biologists have described this early state as one of "cellular indifference," and have ascribed the subsequent differentiation to the variety of cellular environment which ensues as the elements become more numerous. But this way of looking at the facts does not commend itself to Professor L. Bard, who has since 1885 been insisting on what he calls "la spécificité cellulaire." According to this view the various types of cell in the body are like different species with a common ancestor; one cannot be transformed into another; their differentiation is not a function of their environment, but an expression of their inherited properties. Virchow's famous formula has, he says, to be modified into "Omnis cellula e cellula ejusdem naturae."

He admits that his theory has not been welcomed by histologists, but he takes heart in detecting a gradual loss of confidence in the theory of cellular indifference. A final triumph, he tells us repeatedly, awaits his doctrine, and he has no patience with eclectics who would recognise that the early indiffer-

ence is only apparent, or that there is a certain degree of "spécificité." There is no middle way for Professor Bard: it must be yea or nay with spécificité.

In the first chapter of this little book he contrasts the conceptions of indifference and spécificité in a manner which appears to us exaggerated; in the second chapter (on the hereditary fixity of cellular types in adult organisms) he seeks to answer various objections which are suggested by the facts of cellular modifications, of regeneration, of heteromorphosis, etc.; in the third chapter he pursues the analogy of cellular species, and traces their establishment in the course of development; in the fourth he shows how his doctrine bears upon the general problems of biology; and finally there is a list of nineteen publications in which the author has previously dealt with the question.

The new series, of which this book is the first, has for its aim "l'exposé philosophique des faits généraux et des idées directrices nouvelles," but though "la spécificité cellulaire" has evidently been a directive idea to the author, we do not think that he will succeed in convincing many that it is a general fact. To argue the question is not possible within our limits, and we can only express our opinion that the chief interest of the book is as an illustration of ingenious and enthusiastic special pleading in support of a false analogy. We may note in passing that there are a number of irritating misprints, *e.g.* Heckel for Haeckel, and Weissmann for Weismann. J. ARTHUR THOMSON.

A PICTURE-GALLERY OF THE ISOPODA.

An Account of the Crustacea of Norway, with Short Descriptions and Figures of all the Species. By G. O. SARS. Vol. II. Isopoda. Bergen: published by the Bergen Museum. Sold by Alb. Cammermeyer's Forlag, Christiania.

We make no charge to other nations for the use of the English language. This generous extension of free-trade does not pass unrewarded. From time to time it brings us from abroad noble contributions to English scientific literature. It is in our own tongue that we have the satisfaction of reading "An Account of the Crustacea of Norway," by the Norwegian professor, G. O. Sars. His lifelong studies, embracing in turn the several groups of the crustacean class, have given him an almost incomparable facility and trustworthiness as an exponent of them all. The first volume of the "Account," which gave figures and descriptions of all the known Scandinavian Amphipoda, has already been reviewed in these columns. The intelligent reader, which is only another way of saying every reader of *Natural Science*, will recall something of what was then pointed out. It was to the effect that both the large agreement of the Norwegian fauna with our own, and the highly instructive handling of it by Professor Sars, made his work absolutely indispensable to every serious student of the Amphipoda in these islands. A similar remark may be applied to the second volume, just completed, which deals with a second order of sessile-eyed crustaceans, called Isopoda.

The name of this group was given it by Latreille in the Middle Ages, that is to say, nearly a hundred years ago, when people in general knew and cared about crustaceans hardly more than they now know and care about the centre of the earth. The name "isopod" signifies an animal with equal legs, and might therefore include most men and turkeys and many quadrupeds, though not so obviously applicable to the giraffe, the bison, or the kangaroo. But equality between legs, applying to two, or four, is less striking than when it refers to fourteen, a number with which the Amphipoda and Isopoda are endowed. In distinguishing the latter by the character of having equal legs, Latreille chose a name suitable enough to a woodlouse, such as *Armadillidium vulgare*, and to not a few of the marine species, such as *Sphaeroma serratum*, which, like the land woodlouse just mentioned, can roll itself into a neat little pill-like ball.

But since the time of Latreille there have been discovered in the depths of the sea and elsewhere, numerous species of Isopoda, in which the inequality and dissimilarity of the legs attached to one and the same body is carried to an almost extravagant extent. This will be seen at a glance by any one who only turns over the excellent plates with which Sars's work is illustrated throughout. The plates of the present volume are 104 in number. They would often be a kind of revelation to the casual observer, who seeing sees not, as he gazes at the animals themselves in the specimen glasses of a museum. The student also, preparing to dissect a rare isopod, should certainly first make himself acquainted with these drawings before attempting the severance of minute and delicate organs, which may be rendered undecipherable by one rash thrust of an ignorant needle.

As regards classification, it may be remarked that Professor Sars retains the Tanaidae among the Isopoda. This has the great merit of present convenience, whether or not at some future date the separatist party may succeed in detaching this group from its near allies. Most of the species of it, according to Sars, construct for themselves abodes of mud, into which they may wholly withdraw their bodies. On the English coast, however, there is one species pretty frequently to be found, along with the Gribble, in the honeycombing of submerged timber. On the group at large the Professor makes another observation, which is of much interest to the collector. "They all, moreover," he says, "exhibit this peculiarity, namely, that in reaching the surface of the water they remain floating, without being able to re-immerge their bodies, whereby the discovery of the generally very small and inconspicuous specimens is essentially facilitated. On placing some muddy clay taken from greater depths, in a shallow vessel, and stirring up the mud, they will very soon appear floating on the surface, like small white pins, and may easily be taken up for a closer examination." Naturally this mode of discovery will apply to the tenants of mud from small depths as well as great, and in some localities to the sand-dwellers of the sea-shore.

To the elucidation of the tribe Epicarida, it will be found that the work under review has made a very valuable contribution. In this tribe not Alps on Alps, but shrimps on shrimps arise. The Isopoda of which it is formed, in all sorts of insinuating ways, implant and engraft themselves upon other crustaceans, in the process assuming oddities of form, distortions and degradations, in pleasing but often extremely puzzling variety. There is plenty of work apparently still to be done in this branch of investigation, but the intricacies of it have been wonderfully disentangled by the labours, whether in conflict or agreement, of Giard and Bonnier, of Kossmann and of Sars.

In the great variety of species, normal and abnormal, which are shown to belong to the isopod fauna of Norway, it is singular that the Sphaeromidae find no mention. This is a family of extremely extensive distribution in the sea, and is represented even in fresh water. To one of the species incidental allusion was made at the beginning of this notice, simply because it is among the most familiar of British marine Isopoda, so that the absence of the whole family from Norwegian coasts and waters may well cause surprise.

The volume just completed is published by the authorities of the Bergen Museum. To them, therefore, as well as to the author, science is much indebted. There is one small but not unimportant improvement by which they might easily increase the obligation. The seven double parts of the original issue bear dates extending over four years, from 1896 to 1899. When the wrappers, which are of an essentially unstable character, are removed, the bound volume will contain the latter date alone. Since it teems from one end to the other with original observations, and with definitions and descriptions of new genera and species, the reader ought surely to be supplied with some means of ascertaining the true dates of its several parts. This could have been best effected by printing month and year of issue at the foot of the last page of the text,

and on the last plate, in each successive part. The object may be otherwise attained by supplying a continuous list of such pages and plates, to go with the preface or the index. Such a continuous list is in any case desirable, and might still be given for each of the volumes already published. The opportunity of distributing this small boon will be easily provided in company with a far greater one, the promised volume on the Cumacea of Norway, the appearance of which will be for its own sake eagerly welcomed.

THOMAS R. R. STEBBING.

BUTTERFLIES' WINGS.

Specialisations of the Lepidopterous Wing: The Parnassi-Papilionidae. Parts I. and II. By A. RADCLIFFE GROTE. *Proc. American Philosophical Soc.* XXXVIII., 1899. Pp. 25-48, 3 plates.

The author's theory of the movement of the veins of the wings in specialisation suggests a guide for determining the systematic position of the genera with greater exactness, and a clue to their phyletic descent. The correctness of this theory of Grote's has recently received support through Dr. Rebel's discovery of an ancestral form of *Parnassius* from the Miocene of Gabbro, Italy. This extinct species, *Doritites bosniaski*, shows a neuration as yet in the zerynthian stage, and distinctly comparable with that of *Archon apollinus*, while the markings and facies are Parnassian. In this communication to the American Philosophical Society, the author reviews the genera of the Papilionides, showing the Parnassians to be the more advanced forms of the group, the test being the gradual disappearance, through absorption, of the cubital cross vein, as seen by an examination of the generic types from *Ornithoptera* up to *Parnassius*. He is led to the conclusion that the former genus shows generalised characters which bring it nearer to the presumed primitive Papilionid, and necessitate an alteration in the present systematic position of the genus. As opposed to the views of Spuler, the author repudiates any affinity between the Pieridae and Papilionides, and shows that the neurational analogies of the latter group lie with the brush-footed butterflies. The common white colour of the Pierids and Parnassians is ascribed to convergence, and reference is made to the author's earlier statements in *Natural Science*, that an increase of white pigment runs roughly parallel with the specialisation of the neuration. The author further considers and urges the probable diphyletism of the diurnals, as he has previously suggested, and recommends the retention of the Papilionides at the commencement of the series. The plates, in addition to the figures of Papilionides, give corrected figures of *Heliconius*, and for the first time of *Dione*.

COLOMBIAN ORE.

The Ores of Colombia, from Mines in Operation in 1892. By H. W. NICHOLS, S.B. Field Columbian Museum, Publication 33. Geological Series, vol. i., No. 3, pp. 125-177, with Map.

This publication is a praiseworthy endeavour to utilise part of a collection made by Señor F. Pereira Gamba, a mining engineer of Bogota. The collection consists of specimens of the ores and associated rocks met with in those mines of the Republic of Colombia which were being worked in 1892. It was first exhibited in the World's Columbian Exposition, and about a quarter of it was subsequently handed over to the Field Columbian Museum, the remainder having apparently been lost. After an introduction, in which proper stress is laid on the circumstance that all of the specimens of ores are average samples, and after a couple of pages devoted to the physical features and general geology of Colombia, the serious business of recording, and in many cases describing,

the specimens begins. Preceding such descriptions there is in each case a preliminary historical account of the mining done in the district from which the specimens were collected. The sulphide ores are chiefly those of silver, zinc, mercury, lead, and iron, and occasionally of antimony, etc., while large amounts of native gold, silver, and sometimes copper are present in many of the districts. Among the sulphides pyrites is very common, and this is sometimes auriferous. Occasionally copper pyrites, mispickel, stibnite, cinnabar, tetrahedrite, pyrrhotite, enargite, etc., are present. The gold is frequently associated with tellurides. Many interesting examples of paragenesis are given, but to enter into details would be wearisome to the general reader, although they might be perused with avidity by those interested in the mines of this republic, while the mineralogist could not fail to find some useful information in them. One of the most important points with which the author has dealt is the true signification of the names of rocks hitherto employed by former writers when describing these mines. For instance, for the old terms syenite and granite, the author points out that one may generally read andesite or trachyte; "hornblendic material" usually is found to be a rock allied to chlorite schist, and several other examples of the former misapplication of names, owing to lack of the present means for determining the mineral constitution of rocks, will be found in these pages. The author has done useful work in solving some of these enigmas.

In the "General Conclusions," p. 172, he remarks that "the gold and silver ores of Colombia occur either in the acid lavas, which have been erupted at intervals from the close of the Tertiary to the present time, or in Archaean schists in the immediate vicinity of the lavas. In the schists they are usually poor in depth. Owing to the action of the heavy tropical rains, the weathered zone of the deposits has often been greatly enriched, and it was such enriched deposits that gave the immense yields of the early days of Colombian mining."

Three pages, giving the literature relating to Colombian mines, are followed by a map, on which considerable labour has evidently been expended in order to render the topographical details trustworthy. F. RUTLEY.

COCCIDOLOGY.

The Coccidae of Ceylon. By E. ERNEST GREEN. Part I. 1896; pp. i.-xi. + 103, with pls. 1-30. Part II. 1899; pp. xiii.-xli., 105-169, and pls. 31-60. London: Dulau and Co.

The Coccidae constitute an aberrant group of the Hemiptera, contradicting all ordinary definitions of the order and class to which they belong. Hemipterous hexapods, yet in the female sex wingless, and in many genera legless as well. The very methods by which they must be studied are peculiar, and as such distasteful to the ordinary entomologist.

So it has happened that these creatures, though numerous and peculiar, have been greatly neglected. But in recent years, as though outraged by such persistent scorn, they have risen in their might and played havoc with our fruit trees and other crops, not to mention ornamental plants; wherefore we have been obliged to recognise their existence.

Studies usually begun with economic ends in view have led us far afield. It becomes plainer every day that the Coccidae are not only extremely numerous in species, but offer an extraordinary series of peculiar forms, whose organisation, as related to their environment and habits, is of the greatest interest from a purely biological standpoint. The opportunity to advance both economic entomology and pure science is too good to be neglected once perceived; and so we find a new body of students arising, calling themselves coccidologists, and dignifying their study by the name of coccidology.

Of these latter-day students assuredly E. Ernest Green is second to none.

Beginning his researches during the previous decade he at first proceeded slowly. The literature of the subject was difficult to obtain, and when obtained threw little light on the almost unknown coccid fauna of Ceylon. But Mr. Green, not discouraged, resolved to study every species *de novo*, whether described or not; acquiring his knowledge first-hand from nature, as though he might be Adam in the Garden of Eden. This method, in the hands of an intelligent worker, is sure to be successful, and it was eminently so in the case of Mr. Green. He not only prepared descriptions, but also elaborate drawings of every species in all its stages, so far as they could be obtained. This done, a thorough examination of the various publications on Coccidae had to be made before the apparently new forms could be reported as such; but this revealed comparatively few identities. In Part I. there are thirty species described, of which seventeen were new; in Part II. are twenty-nine species, all but three discovered by the author. It often happens, on receiving a paper describing so many new species, that one can immediately detect some synonymy; but I do not know of any "bad species" among the forty-three above mentioned, and doubt if there are any. The descriptions are good, and the plates most beautiful. There is an introductory portion on the general principles of coccidology, including a new classification of the sub-families and full directions for collecting and preserving material. There is also a chapter on the insecticides and other means for destroying Coccidae which injure cultivated plants. In the last-mentioned chapter the interesting fact is brought out that practically all the injurious coccids in Ceylon are those described from elsewhere and presumably introduced into the island. The truly native species, almost without exception, have proved to possess no economic importance; though of course these very species, *carried somewhere else*, may yet become notorious.

An unfortunate conservation, as it seems to the present writer, is shown in regard to genera. The species assigned to *Aspidiotus* represent at least five very distinct groups, which are at least of sub-generic value. *Aonidia* is made to include very diverse forms, including three distinct generic types. Similarly the twenty-six species assigned to *Chionaspis* are by no means truly congeneric; for instance the first six, *aspidistrae*, *theae*, *albizziae*, *mussaendae*, *rhododendri*, and *scrobicularum*, belong properly to *Hemichionaspis*. The generic classification of the Coccidae, however, is at present in a transitional state, and an author cannot be blamed if he hesitates to propose changes while yet uncertain what those changes should be.

Simply as an illustration of good methods this work ought to be examined by zoologists who do not expect to study Coccidae; while for the coccidologist it is of course essential. Under these circumstances it is to be regretted that the manner of publication is such as to make it extremely costly. The only edition has coloured plates, which of course are expensive, while the colouring does not greatly add to their value for scientific purposes. It would have been excellent to have a coloured edition of small size, if there could have been an uncoloured one at a more moderate price. But the chief trouble is that the publishers insist upon receiving the full subscription (£5) for the work in advance, though it must take at least several years to complete it. The work, of course, is intrinsically worth all that is asked for it, and more; but the fact remains that zoologists are not commonly blessed with superabundant means, and are reluctant to part with a five-pound note under the circumstances just mentioned. Surely if the conditions of sale were rendered easier the subscriptions would become so much more numerous that the amount received would be considerably greater than at present.

T. D. A. COCKERELL.

MULTUM IN PARVO.

Insects: their Structure and Life. A Primer of Entomology. By G. H. Carpenter. Small 8vo, pp. viii. + 404, with 183 illustrations. London: J. M. Dent and Co. Price 4s. 6d.

In this book the author traverses most of the very wide field of the division of zoology he is dealing with. Although the great extent of his subject prevents him from discussing moot points in detail, yet he succeeds in giving a very fair general idea of the present state of entomological science and of the subjects that have been predominant in the discussions of the last twenty-five years.

If any fault is to be found with the book it is that its subject has been extended unduly by the inclusion of matter that is not specially entomological. The chapter on the classification of insects is chiefly devoted to natural selection, causes of variation, and kindred topics. These matters are, however, set forth in a spirit and manner that no one can object to; and, though their predominance is scarcely consistent with the title of the work, it is probable that one of the author's objects was to show the bearing of entomology on these more general subjects. We hope that the work will find many readers, and that most of them will approve the wide view the author has taken of his subject.

The book is copiously illustrated and well got up. Most of the 183 illustrations, called figures, are really combinations of numerous figures. They have been well selected, and their execution, except in a few cases, is satisfactory. There is also a very useful and sufficiently extensive bibliographic list, and good index. Altogether, the work may be strongly recommended to purchasers who wish to give only a small sum for a trustworthy introductory work on this subject. They will receive excellent value for their money. D. S.

THE MAMMALS OF FRANCE.

Faune de France—Mammifères. By A. Aclogue. 12mo, 84 pp. Paris, 1899.

To treat adequately of the mammals of France within the compass of eighty-four duodecimo pages, especially when a large portion of the space is occupied by introductory matter and illustrations, would seem an almost impossible task. Nevertheless, with the assistance of irritatingly minute type, and much "boiling-down" of matter, Monsieur Aclogue has succeeded not only in enumerating all the species, both wild and domestic, met with in France, but also in giving the leading characteristics of both genera and species, as well as of the larger groups.

Nothing, however, is said as to habits, and but little in regard to the details of local distribution and variation. And as the two latter features are those alone which would render the work of importance to zoologists of other countries, it can scarcely be said to be altogether satisfactory.

As it would appear from the title that the complete work is intended to include the entire fauna of France, the author may perhaps be induced to pay more attention to these points in subsequent parts. The character of the illustrations might, too, be improved with advantage. And there is likewise room for some amendments in nomenclature; *Arvicola*, for instance, being retained for the voles, while the martens figure as *Martes* in place of *Mustela*. Still, in spite of its imperfections, it is useful to have a work containing all the representatives of the French mammalian fauna.

A CLASSIC FOR CLIMBERS.

Hours of Exercise in the Alps. By JOHN TYNDALL, LL.D. Pp. i.-xii. and 1-482, with seven full-page illustrations. New Edition. London: Longmans, Green, and Co., 1899. Price 6s. 6d.

This new edition of a highly characteristic work is practically the reprinting of a classic. So far as the compositors' work could allow, it is more—it is an actual reproduction. A full index has been wisely added, a matter on which the author was strangely indifferent; even his popular "Forms of Water" appeared without one. A few notes by L. C. T., bringing certain statements up to date, have been made with conscientious care.

The book was first issued, by the same publishers, in 1871, and must have been written with as much enjoyment as it has again and again brought to others. The human personality of it must always remain fresh; and climbers will not tire of details of these earlier exploits. Switzerland, the middle level occupied by church-congresses, university-extension parties, and the host of unattached or exploited tourists, has changed conspicuously in the last thirty years; but the great peaks and snow-girt amphitheatres remain for the men of firm nerve, of resolute and confident resource. Such men, year after year, bring to the snow-slope and the *arête* the quickness of judgment and the orderly perception which have made them masters in their own professions, masters alike of human prejudice and of mountain-barriers. Whether all such will approve the school-boy rashness of some of Tyndall's joyous escapades, none can fail to respond to his enthusiasm, or to smile with him in his hours of success. The story of the rescue of the porter on p. 144 touches a far graver note. The book concludes with several short papers, among which is a considerable discussion on regelation. On p. 421 we have the well-known account of a winter ascent of Snowdon, in the company of Professor Huxley. Though a number of Alpine climbers have since exercised themselves on the crags of Lliwedd and Crib-goch, how many of the English tourists who throng Grindelwald or Zermatt have seen Wales under any other covering but that of August rain? A journey from London to Llanberis, in the crisp clear days of January, will soon make even an ordinary walker share the enthusiasm of our author.

To say "our author" is not in this case a convention; there is much in this volume, even in its simplicity and candour, which must seem to all of us like the cheery handshake of a friend. G. A. J. C.

EXPERIMENT IN GEOLOGY.

La Géologie expérimentale. By STANISLAS MEUNIER. Pp. i.-viii. and 1-312, with 56 illustrations. Paris: Felix Alcan, 1899. Price 6 francs.

This compact little book, forming a volume of the "Bibliothèque scientifique internationale," summarises its author's researches, which have extended over thirty years, much as those of M. Gaudry were aptly brought together in "Les Ancêtres de nos animaux." The author, possibly from a desire for dispassionate exposition, has chosen to write in the third person. This is the very opposite of the method of the late Professor Tyndall; and the middle tone of partial self-suppression adopted by authors of average literary gifts probably represents the canon of taste in dealing with one's own observations. Mr. Stanislas Meunier's plan has the disadvantage of reminding us of the handbill issued by Mr. Samuel Gerridge in Robertson's comedy of manners. Apart from this, the descriptions are of course clear and definite, in the admirable fashion of French text-books; and the discussions that are involved, as well as the replies to criticism, are never unduly extended. The instruments devised, and the permanent results obtained, have been formed into a collection in the

geological gallery of the Jardin des Plantes in Paris. The book thus provides an agreeable guide to this collection.

There is much in it that will be of service to the teacher of ordinary classes, such as the reproduction of earth-pyramids, described on p. 40, and of sand-dunes, described on p. 210; while the broad and at times generous deductions from the experiments deserve the attention of the physical geographer as well as the geologist. The discussion (pp. 107-111) of the continuous diminution of glaciers by the continuous erosion of their gathering-grounds and of their beds, is an example of how the larger natural features are always present to the mind of the experimenter.

The book is brief; yet there is frequent mention of the work of other authors. It is impossible, in such limits, however, that such reviews of previous observations should be complete. As an expression of Mr. Stanislas Meunier's own work and of his own conclusions, the volume is especially profitable to the reader. Towards its close, we touch on the great questions of igneous magmas and metamorphism, which are now agitating the geological world. We commend the bold suggestion made on p. 266, to those who regard the solution of one rock in another as confined to contact-phenomena. The author here derives the water required for volcanic action from the absorption of blocks of the water-logged outer layers of the crust by the molten and anhydrous mass below. G. A. J. C.

We have received *Naturae Novitates* for 1898, that useful fortnightly bibliographic bulletin of natural science issued by Messrs. Friedländer of Berlin. The collected parts for 1898 amount to 780 pages, the index occupies about 90, the number of citations is 9359, and the price is only four marks. It is now in its twenty-first year of issue, and deserves to be congratulated on attaining its majority.

In the September number of *The Naturalist* there are obituary notices of Mr. John Cordeaux by W. Eagle Clarke and the Rev. E. A. Woodruffe-Peacock. A note by E. Whitehouse points out that *Hydra viridis* devours Aphides greedily. "The Hydra would thus be very serviceable in a greenhouse if they could live on plants."

The *Zoologist* for August 15 contains an obituary and portrait of the late Sir William Henry Flower, and a continuation of Mr. W. L. Distant's lively paper on mimicry.

Knowledge for August contains a continuation of the anthropological studies by Prof. Arthur Thomson of Oxford, and the tenth instalment of Mr. Stebbing's "Karkinokosm," which reads like a novel. A striking photograph of proboscis and snub-nosed monkeys illustrates a lively paper by Mr. Lydekker, entitled "A Contrast in Noses."

The *American Naturalist* for August contains articles on the Hopkins Seaside Laboratory, by Prof. Vernon L. Kellogg (*see* "Notes and Comments"); on the North American arboreal squirrels, by Mr J. A. Allen; and on an abnormal wave in Lake Erie, by Mr. Howard S. Reed. There is also an obituary of Dr. Alvin Wentworth Chapman, by Prof. W. Trelease, and a synopsis of North American Gordiacea, by Dr. Thomas H. Montgomery, jun.

Knowledge for September contains, *inter alia*, a fifth paper on the Mycetozoa, by Sir Edward Fry; a popular essay on Fairy Rings, by Mr. A. B. Steele; the beginning of an account of Ben Nevis and its Observatory, by Mr. W. S. Bruce; a paper on Clouds (with good photographs), by Messrs. E. M. Antoniadi and G. Mathieu; and a letter by Dr. C. S. Patterson adversely criticising some of the conclusions in Prof. M'Intosh's "Resources of the Sea."

In a short paper contributed to the *Mt. Ges. Bern* for 1897, Dr. A. Girtanner describes a fine piece of horn-cores of the European Ibex obtained from the pile-village of Greny on the Martensee. It appears that in cavern deposits, the farther we depart from the Alps the rarer become the remains of the Ibex, and hitherto the horn-cores have only once been found in a Swiss lake-village. From these facts it has been inferred that the animal was always a mountain-dweller. In the present instances the author compares the ancient horn-cores with modern horns, much to the disadvantage of the latter.

Science for September 1 contains the following interesting note:—"The American word 'scientist,' proposed by the late Dr. B. A. Gould, is apparently becoming acclimatised in Great Britain. Though *Nature* has stated that the word is excluded from its columns, it has occurred in the editorial notes. It will also be found in the *Academy* and in the London *Times*. The latter, in the issue of August 15, even uses the word retroactively, speaking of 'the great German scientists of the past.' But the best testimony that the word must now be regarded as correct and classical English, is the fact that it is to be found in Mr. Thomas Hardy's 'Two on a Tower.'"

The *Photogram* for July has a translation from *Le Photo Gazette* of an interesting brief article by Fabre-Domergue on photographing aquaria by magnesium flash-light. He experimented at the laboratory of marine zoology at Concarneau, and got some good results, a specimen of which is given. The magnesium light produces a lively effect on the fishes in the aquarium, but the reflex movement is relatively slow, and the light is gone before it takes place.

We have received the September number of *The Westminster Review*, which contains two articles involving biological considerations—one against the Contagious Diseases Acts, by Ellis Ethelmer, and another containing a suggestion of a substitute for the marriage laws, by Herbert Flowerdew. The latter says: "Let us suppose, then, that the legal marriage contract consists simply in an agreement between man and woman to live together until such time as either chooses to terminate the agreement, and to be jointly responsible for the maintenance of the children born during the arrangement and within nine months of the termination, both parties agreeing to compensate the other for any loss incurred by his or her failure to make the arrangement permanent."

The September number of *Science Gossip* contains, among other articles, one by Major H. A. Cummins on Sikkim, "a veritable paradise for the naturalist, be he botanist, zoologist, or geologist, but especially for the botanist"; and a continuation of papers on British freshwater mites, by C. D. Soar; on the collection and preparation of Foraminifera, by A. Earland; on ticks and louping-ill, by E. G. Wheeler; on palaearctic butterflies, by H. C. Lang; on chalk, by E. A. Martin; and on meteorites, by John T. Carrington.

Prof. L. V. Pirsson of Yale succeeds the late Prof. Marsh as an editor of the *American Journal of Science and Arts*.

The journal of the Straits branch of the Royal Asiatic Society for June 1899, which has been sent to us, contains some interesting papers. The list of birds of the Larut hills, by Mr. A. L. Butler, has some interesting field notes. Bishop Hose gives a list of the ferns of Borneo, and Mr. H. N. Ridley a list of the scitamineae of the Malay Peninsula. A pleasant little journey into an unexplored corner of Pahang is graphically described by Mr. W. B. Roberts. The most important paper, however, is one by Mr. Ridley on the habits of Malay reptiles. It contains many valuable observations which it is to be hoped will find their way into our books on natural history. The society may be heartily congratulated on the production of this volume, which indicates a considerable activity in biological work in the Straits Settlements.

OBITUARIES.

JOHN CORDEAUX

Born, 1831; Died, August 1, 1899

John Cordeaux was the eldest son of the Rev. John Cordeaux, formerly rector of Hooton-Roberts, Yorkshire. He was born at Foston Rectory, in Leicestershire. The ordinary occupations of a country life gave him opportunities for the especial study of bird life, with which his name has been so long associated. Among his earliest publications was one on the birds of the Humber district, which has remained the standard for that district. Ever keen on the migratory habits of birds, he strove hard to interest the coast-guard and lighthouse-keepers to keep continuous records at their various stations, and succeeded in obtaining a great variety of useful and interesting data, both on the migratory habits of our own birds and the occasional visits of strangers. Nor did he neglect other interests, for he was an enthusiastic botanist, archæologist, and student of folklore. Cordeaux was first president of the Lincolnshire Naturalists' Union. We are indebted to the *Times* for the facts in the above notice.

The following deaths are announced:—The entomologist Perez Arcas in Madrid; Dr. Daniel Garrison Brinton, the American ethnologist, at Atlantic City, New Jersey, on July 31, at the age of 62 years; the botanist Eugène Gonod d'Artemare, at Ussel (Corriège), on June 16; the psychologist Freiherr Karl du Prel, who made a number of contributions to evolution-literature, at Heiligkreuz in the Tyrol, on August 5, at the age of 60; Dr. Pasquale Freda, director of the agricultural experiment station at Rome, on July 4; Mr. N. R. Harrington, member of the Senff Zoological Expedition, instructor in Western Reserve University, at Atbara, July 27; Charles Howie, of St. Andrews, who published a list of the mosses of Fife and Kinross; St. Th. Jakčič, professor of botany and director of the botanical garden at Belgrad, on May 4; the French geologist Adolphe Legeal, murdered in the Soudan; Dr. Joseph Mies, anatomist and anthropologist, on June 9, in Köln; A. de Marbaix, professor of zootechnic at the Catholic university of Louvain, at Meerhout, near Antwerp, on August 5, in his 74th year; Robert H. Schmitt, geographer in German East Africa, on May 10, in Mangali, at the age of 29; the mycologist Johann N. Schnabl, in München, on June 16, at the age of 45; Henri Lévèque de Vilmorin, a notable cultivator of plants, first vice-president of the Paris Société d'Horticulture.

NEWS.

The following appointments have recently been made:—Dr. A. P. Anderson, assistant professor of botany in the University of Minnesota; Dr. O. Appel as an assistant in the biological department of the Agricultural and Forestry Institute in Berlin; Mr. Carlton R. Ball, assistant in the division of agrostology, U.S. Department of Agriculture; Dr. W. Benecke, as docent for botany at Kiel; Dr. Alfred Bergeat as professor of mineralogy and geology in the Mining Academy at Clausthal; Mr. H. Blodgett, an assistant botanist and entomologist in the New York Branch Agricultural Station at Jamaica; Dr. Franz Boas, as professor of anthropology in Columbia University; Prof. E. A. Burnett, of the Agricultural College of South Dakota, to the chair of animal husbandry in the University of Nebraska; Dr. A. Cancani, seismologist, to succeed Dr. G. Agamenone as assistant in the central office of meteorology and geodynamics at Rome; Dr. Chatin, as professor of histology at Paris; Prof. Alessandro Coggi of Perugia, as professor of zoology in the University of Sienna; Dr. N. K. Czermak, as professor of anatomy, histology, and embryology in the University of Dorpat; Mr. H. N. Dickson, as lecturer on physical geography in the New School of Geography in the University of Oxford; Dr. von Elterlein, docent for mineralogy and geology in Erlangen; Dr. Enrico Festa, assistant in the zoological museum of the University of Turin; Prof. Max von Frey of Zürich to be professor of physiology at Würzburg; Dr. Alberto Fucini, as docent for palaeontology and geology in the University of Pisa; Dr. Ercole Giacomini of Sienna, associate professor of zoology in Perugia; Mr. A. W. Gibb, to be university lecturer on geology in the University of Aberdeen; Dr. Hugo Glück, docent for botany in the University of Heidelberg; Dr. Guitel, as adjunct professor of zoology at Rennes; Mr. Eustace Gurney of New College has been appointed to the Oxford University table at the Naples Station; Dr. W. H. Hobbs, assistant professor of mineralogy and petrology at the University of Wisconsin, to a full professorship; Dr. A. C. Houston, as lecturer on bacteriology at Bedford College, London, for women; Abel A. Hunter, as botanical collector for the University of Nebraska; Dr. A. Jakowatz, as demonstrator in the botanical museum in Vienna; Dr. J. Janse, as director of the botanical gardens at Leiden; Dr. K. Keissler, as assistant in the botanical museum in Vienna; P. Beveridge Kennedy, assistant in the division of agrostology, U.S. Department of Agriculture; Dr. Max Koch, as titular professor of geology in Berlin; Dr. H. B. Kümmel, assistant professor of physiography at Lewis Institute, Chicago, has been appointed assistant state geologist of New Jersey; Dr. Künstler, as professor of comparative anatomy and embryology at Bordeaux; Dr. A. C. Lane, to be state geologist of Michigan; Albert Lindström, as honorary professor of anatomy in the Karloninian Institute in Stockholm; Dr. Günther Beck von Mannagetta, as professor of systematic botany in Prag; Dr. Johannes Meisenheimer, as privat docent in embryology in the University of Marburg; Elmer D. Merrell, assistant in the division of agrostology, U.S. Department of Agriculture; G. T. Moore, as instructor in botany at Dartmouth College, U.S.A.; W. A. Orton, assistant in the U.S.

Department of Agriculture; A. J. Pieters, as first assistant in botany in the Department of Agriculture at Washington; Dr. Antonio Porta, to be assistant in the zoological museum of the University of Parma; Prof. C. S. Prosser, of Union College, Schenectady, New York, as associate professor of historical geology at Ohio State University, Columbus; Dr. Francis Ramaley, of the University of Minnesota, to be professor of biology in the University of Colorado, at Boulder, in succession to Prof. John Gardiner, retired; Dr. A. L. Rimbach, as *ad interim* instructor in vegetable physiology and pathology in the University of Nebraska, to allow Prof. Bessey to give the necessary time to the duties of acting chancellor; Dr. Friedrich Schenck, as professor extraordinarius of physiology in the University of Würzburg; Hermann von Schrenk, as a special agent of the Department of Agriculture at Washington to study diseases of trees; James Y. Simpson, M.A., B.Sc., lecturer on natural science in the Free Church College, Glasgow; Dr. Otto Stapf, as chief assistant in the Kew Herbarium; H. G. Timberlake, as instructor in botany in the University of Wisconsin; Dr. Weinschenck, as docent for mineralogy and geology in the Technical School of München; Dr. Karl Wenle, as directorial assistant in the ethnological museum in Leipzig.

The Council of the University of Melbourne will shortly appoint a professor to the chair of Geology and Mineralogy. The professor is expected to devote the whole of his time to the work of his department, and will be required to deliver two courses of lectures of three hours a week each, and to undertake the training of students both in field and laboratory work. The salary of the professor is £1000 per annum, but in the event of the Council providing him with a residence in the University grounds, the sum of £100 per annum will be deducted from his salary as aforesaid. The University has a fair collection in palaeontology and mineralogy, but has no specially fitted up laboratory for geological work. A suitable room in the University buildings will be provided in which to organise this part of the work. Lectures begin in 1900, on Thursday, March 1. The salary of the office will commence from the 14th February 1900, or from the date of the professor's arrival in Melbourne, if later than the 14th February. If the professor appointed come from Britain or America, £100 will be allowed for travelling expenses. Applications for the vacant chair must be sent to the office of the Acting Agent-General for Victoria, 15 Victoria Street, Westminster, London, by October 20, 1899.

At a meeting of the General Committee of the British Association on September 15, Professor Sir William Turner was elected president of next year's meeting of the Association, to be held at Bradford.

A. Targioni-Tozetti has been elected an ordinary fellow of the Reale Accademia dei Lincei in the Department of Agriculture; A. Borzi as a corresponding fellow in the same department, and F. Delpino for botany. The list of foreign fellows includes—for geology and palaeontology, O. Torell, A. de Lapparent, and R. Lepsius; for botany, W. Pfeffer; for zoology, Ernst Haeckel and E. van Beneden; for physiology, E. Pflüger and E. Hering—a notable list, but without any British representative.

Prof. E. Pflüger, the famous physiologist of Bonn, recently celebrated his 70th birthday.

Prof. C. J. Herrick of Denison University has been awarded the Cartwright prize of $500 by Columbia University for his work on the brain of fishes.

The Académie Internationale de Géographie Botanique has conferred its international scientific medal upon Prof. John M. Coulter.

It is noted in *Science* that the Alvarenga prize of the College of Physicians of Philadelphia has been awarded to Dr. Robert L. Randolph of Baltimore for his essay entitled "The Regeneration of the Crystalline Lens: An Experimental Study."

On the occasion of the ninetieth anniversary of the foundation of the University of Berlin (at the beginning of August) Prof. W. Waldeyer discussed the question, "Does the University of Berlin fulfil the mission entrusted to it by its founder?" but he confined himself mainly to the progress of the anatomical department.

From an analysis published in *Science* for August 4, it may be seen that of the doctorates granted by the United States Universities this year, 32 were for chemistry, 7 for physics, 5 for geology, 4 for palaeontology, 11 for botany, 11 for zoology, 15 for psychology, and so on. "It may be noted that at Johns Hopkins more than half the scientific degrees are given in chemistry. This science also leads at Yale and Harvard. Psychology and education are especially strong at Columbia. Chicago stands first in zoology and in physiology."

According to the Allahabad *Pioneer Mail*, cited in *Nature*, Mr. J. N. Tata's munificent offer to endow a Scientific Research Institute in India has now been dissociated by the generous donor from the proposed family settlement, which was one of the original conditions.

By the will of the late Dr. Jules Maringer, the Pasteur Institute at Paris will receive 100,000 francs.

Science reports the following gifts and bequests:—$1000 from Mr. Emerson M'Millin to the research fund of the American Association; about $50,000 to Yale University, by the will of the late Dr. C. J. Stillé; £10,000 to Glasgow University, by the will of the late James Brown Thomson.

We learn from the *American Naturalist* that Columbia University has recently received $10,000, to be known as the Dyckman Fund, the interest of which will be used in the encouragement of biological research on the part of graduate students.

We learn from the *Botanical Gazette* that the extensive botanical library and herbarium accumulated by the late Prof. D. C. Eaton of Yale have been given to the University by his family, and that a graduate scholarship in botany has been founded by his widow.

Science publishes the letter in which Prof. C. E. Beecher offers as a gift to the Peabody Museum of Yale University his entire scientific collections, which represent twenty years of personal work, and comprise upwards of one hundred thousand specimens. The collections represent (1) the fauna of the Upper Devonian and Lower Carboniferous in Pennsylvania; (2) the fauna of the Middle Devonian of Western New York; (3) the fauna of the Lower Devonian of Central and Eastern New York; (4) a small series from other geological horizons; (5) about five hundred type specimens. There are hundreds of specimens unique for their perfect preservation and for their careful preparation to show delicate structural details. No other single collection in America is so rich in series, showing the life-histories of species from the embryonic to the adult state.

A course of twelve free lectures on the "Pleistocene Mammals" will be delivered by Dr. Ramsay H. Traquair, F.R.S., in the Lecture Theatre of the Museum of Practical Geology, Jermyn Street, S.W., on Mondays, Wednesdays, and Fridays, at 5 P.M., beginning Monday, October 2, and ending Friday, October 27.

In the *Scientific American* for August 12, Miss Alice Dinsmore gives a lively account of Nature-study in the Summer School of the College of Agriculture of Cornell. There were three departments—the study of insect life, directed by Prof. Comstock; plant life, directed by Prof. Bailey; and farm

life, directed by Prof. Roberts, the instruction in each case being eminently practical. The course is attended chiefly by teachers, and the report gives an impression of sound and thorough work.

The new lecture hall of the American Museum of Natural History is expected to be ready next month. It will seat 1700.

We learn from our esteemed contemporary, the *American Naturalist*, that the state of Wisconsin has appropriated $10,000 for two years for a geological and natural history survey of the state, under the direction of Prof. E. A. Birge. Some of these "appropriations" may be contrasted with, for instance, the apparent impossibility of getting Government support for the survey of Scottish lakes. But the subsidy for the Antarctic Expedition must silence our grumbling in the meantime.

The fine collection of Scottish agates made by the late Prof. Heddle is now arranged in the Museum of Science and Art in Edinburgh. Mr. J. G. Goodchild has prepared a guide to the collection, incorporating Prof. Heddle's explanatory notes.

According to the *Scientific American*, the creation of a great national forestry and game reserve in northern Minnesota, embracing 7,000,000 acres around the headwaters of the Mississippi River, with many lakes of rare beauty, well stocked with fish, will be advocated before Congress next winter by prominent citizens of Chicago and Minnesota. The promoters of the plan are not likely to experience much difficulty in interesting Congress. The game and the virgin forests of the United States are disappearing so rapidly that it is exceedingly important that measures be taken, before it is too late, to save some of the great wooded areas of the continent.

The report of the South African Museum for 1898 by the Director, Mr. W. L. Sclater, gives details as to the growth of the collections. The rocks of the Kimberley mining district have been arranged and displayed, and considerable progress has been made with the collection of South African mammals.

The recently-published British Museum blue-book takes account of the additions to the Natural History collection during 1898, *e.g.* the first instalment of the Norman collection of marine invertebrates, the rare mollusc *Pleurotomaria beyrichii*, the rare fossil Elasmobranch *Squatina alifera*, the late Rev. P. B. Brodie's collection of fossil insect remains, the Piper collection of fossils from the strata of the Ledbury tunnel, and a selection from the late Rev. T. T. Lewis's collection of Old Red Sandstone fossils.

It is noted in *Science* that Dr. A. B. Meyer, the energetic director of the museums in Dresden, is now in the United States inspecting American museums before the new buildings in Dresden are erected.

We learn from the *Victorian Naturalist* that the desirability of removing the National Museum at Melbourne to a more central and accessible site was affirmed at a meeting of the trustees on June 1, and that Professor Baldwin Spencer was appointed honorary director in succession to the late Sir Frederick M'Coy.

It is reported in *Science* that the Boston Public Library will undertake the publication of a card catalogue of physiology with brief abstracts, under the editorship of Prof. W. T. Porter of Harvard Medical School.

We learn from *Nature* that the city of New York has allocated 63,000 dollars for the zoological garden in Bronx Park, and that it is proposed to raise the appropriation for the American Museum of Natural History from 90,000 to 130,000 dollars annually.

It is noted in *Science* that the Ahearn bill, recently passed by the New York legislature, allows $96,000 to be spent next year on free lectures, largely scientific, in New York City.

In *Nature* for August 31, Prof. A. C. Haddon gives an interesting preliminary report on the results of the Cambridge Anthropological Expedition to Torres Straits and Sarawak. He and the other members of the expedition deserve congratulations on the successful issue of their explorations, and the detailed memoirs will be awaited with interest.

The Liverpool expedition for the study of malaria in Sierra Leone included Major Ross and Dr. Annett of the Liverpool School of Tropical Diseases, Mr. E. E. Austen of the British Museum (Natural History) as entomologist, and Dr. S. Van Neck, official delegate of the Belgian Government.

Major Ronald Ross, leader of the expedition, has already succeeded in finding the malaria-bearing mosquito.

It is stated in *Nature* that Mr. J. S. Budgett of Trinity College, Cambridge, has been successful in obtaining eggs and larvæ of *Polypterus.*

Mr. George K. Cherrie has returned from his expedition to Venezuela, where he was collecting birds for the Tring Museum. Although his work was stopped by illness, he got many spoils.

The steamship *Capella* arrived at Tromsö on August 18 from Franz Josef Land, bringing Mr. Wellmann's expedition from Cape Tegetheff. It is reported that the expedition reached the 82nd parallel. Some important scientific work was done and 103 walruses were killed. Mr. Wellmann has been unfortunately crippled by falling into a snow-covered crevasse. He has now returned to Britain.

We learn from the *Botanical Gazette* that Mr. J. N. Rose has just returned from a botanical trip in Mexico, where he rediscovered *Echinocactus parryi*, and collected other species lost or hitherto unknown to American herbaria. He made a thorough study of the species of agave, especially those used in the manufacture of pulque and mescal. He visited, among many other localities, Tequila, in order to find out what plant furnishes "tequila," which is the great mescal drink of Mexico.

The *Scientific American* notes that Dr. Frederick W. True of the Smithsonian Institution, a well-known authority on cetaceans and seals, went in August to Newfoundland to hunt finback whales, in order to obtain specimens for the National Museum at Washington.

The workers on board the Prince of Monaco's *Princess Alice*, which has just returned from a Spitzbergen cruise, were able to do some surveying work, and a large unsuspected bay was discovered. Investigation was much hindered, however, by the vessel running on to rocks, where she remained for five anxious days. Liberation involved a loss of most of the coal, and this forced the Prince to return sooner than he would otherwise have done.

Mr. Benjamin Hoppin has sent his yacht *Senta* to Greenland as a gift to the Peary Relief Expedition, with the sole restriction that he wishes it to be used in scientific exploration.

The *Scientific American* reports that the party of scientific explorers who went to Alaska as the guests of Mr. Harriman, met with success, and made several important discoveries. Among these was an immense bay extending inland for over twenty miles. At the upper end of this bay they discovered a great glacier inferior only to the Muir glacier in size. Several other new glaciers were discovered. Some new plants were found by the botanists, and the collection of marine species is expected to surpass any yet made in northern waters.

We learn from *Science* that Prof. Wm. Libbey and Dr. A. E. Ortmann of Princeton University have gone to dredge and explore in Inglefield Gulf on the steam whaler *Diana*, of the Peary Relief Expedition.

Dr. Robert Logan Jack, late Government Geologist for Queensland, and special commissioner in charge of the exhibits at the Greater Britain Exhibition, has accepted an appointment from Mr. Pritchard Morgan to run some mining concessions in Szechuan, Korea, and North China. Dr. Jack sailed in September.

Dr. J. B. Hatcher, of the Zoological Department of Princeton University, has returned from his expedition to Patagonia, and some account of his results is to be expected shortly in *Science*.

Prof. John B. Smith gives in the *Scientific American* an interesting account of "an improved method of studying underground insects"—by pouring liquid plaster of Paris into the burrows and digging out the cast after it has set. As he says, "concerning the habits of underground insects we are yet greatly in the dark, and much of our supposed knowledge is really inference from observations made upon the insects when at the surface, or from such excavating as has been done in attempting to follow out the burrows of diggers." It may be recalled that the Duke of Argyll used the plaster of Paris method in studying the burrows of the lugworm.

The forty-fourth annual exhibition of the Royal Photographic Society, at the Gallery of the Royal Society of Painters in Water Colours, 5A Pall Mall East, was opened to the public on Monday, September 25, for a period of seven weeks.

The Tsar has recently ordered the allotment of three million roubles to found boarding-houses for University students; and this has been followed by a Government proposal to establish and subsidise boarding-houses, scholarships, and even tutors for the children of the provincial nobility who are attending the middle-class schools.

It is noted in a recent issue of the *Scientific American* that while Britain stands first in the production of slate, and France comes a good second, it will soon be necessary to place the United States well up in the list of competitors. The estimated world production is valued at 16 million dollars, of which $8\frac{4}{5}$ millions fall to Britain, and over $3\frac{1}{2}$ to the States.

The *Times* gives, from a report of the British Consul at Naples, an interesting account of the island of Procida, in the Bay of Naples. Unlike Ischia and Capri, it is very rarely visited by travellers, though in point of scenery it is almost superior to them. It is about two miles long by one mile broad, and carries the enormous population of 14,000 souls. Its sailors are the best in the Bay of Naples, and its little harbour is usually thronged with sailing vessels, which do the coasting trade of Italy and the neighbouring islands. The plateau in the centre of the island produces excellent wine and fruit. Some of the people manufacture very fine gut from silkworms. They call the product *fili di seta*, or "silk threads," the special properties consisting in strength and flexibility. They are made from the stomachs of silkworms just before they begin to spin their silk and form their cocoons. Not many worms in proportion to the amount of gut put on the market are reared in Procida itself, but the makers buy them from Torre dell' Annunziata and other neighbouring towns in great quantities. The worm when fully matured, that is, at the moment when its nourishment ceases, and just before its metamorphosis, is cut open, great care being taken not to injure the membrane of the stomach. This is then removed, and usually reaches the length of 13 to 20 millims., with a diameter of $1\frac{1}{2}$ to 2 millims. The stomachs are then put into a pickle, the secret of

which is carefully kept. When the pickling process is over, the workpeople, who are mostly women, take one end of the stomach in their teeth and draw the other end with their hands. This part of the work requires great dexterity, for the threads are drawn out to the length of 30 to 50 centims., and the whole value of the product depends upon its length in relation to its thickness, and the strain it will carry. There are two seasons for the production—spring, when the best gut is produced, and autumn, when the quality is inferior. There is an important market for this speciality, and the whole production is exported to Northern Italy and abroad at the average price of 150 lire per kilo. The gut is very light, so that a great deal of it goes to a kilo. The cost of production is also considerable, as the worms must be bought just at the moment when they are becoming profitable for making silk, that is when they are at their dearest. Again, the results are frequently disappointing, many worms being found, on dissection, not to be suitable, and having to be discarded.

The *Scientific American* refers to some statistics recently published by the French Meteorological Bureau at Paris. Spain has 3000 hours of sunshine a year; Italy 2700; France 2600; Germany has 1700, while England has but 1400. The average fall of rain in the latter country is greater than that in any other European country. In the northern part and on the high plateaus of Scotland about 351 inches of rain fall a year, and London is said to have an average of 178 rainy days in the year, and fully ten times the quantity of rain that falls on Paris.

In reference to a note which was recently published in our columns on the difficulty of inoculating locusts with fungus owing to the frequent moults, it is interesting to see that the recent experiments at the Cape have proved very effective.

According to Spring's experiments, reported in the *Scientific American* of September 2, a pure blue is the natural colour of water. Finely divided white or colourless particles reflect a yellow light, which unites with the natural blue to form a bright green. The fact that the water of ordinarily green lakes turns perfectly colourless at times, is not due to a clarification, but, on the contrary, to an influx of a reddish mud, coloured by ferric oxide, which completely neutralises the green.

An interesting experiment is being made by the Government of Bosnia and Herzegovina in connection with the subject of the migration of birds. A number of observatories are being established all over these two countries, on the coasts, plains, mountains, rivers, and lakes—in fact, in every spot which seems likely to yield results of interest to those engaged in researches on bird migration. Under the auspices of the Government of the two countries named, a meeting of ornithologists was convened at Sarajevo from the 25th to 29th of September with a view to similar observations conducted on uniform methods being instituted elsewhere. A report was presented on the bird life of the Balkan States, illustrated by a fine collection from those districts.

Natural Science

A Monthly Review of Scientific Progress

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NOTES AND COMMENTS.

Disturbing the Balance of Nature.

No one who has appreciated the reality of the struggle for existence is likely to be in haste to disturb the balance of nature either by eliminating old-established inhabitants from an area in which they have settled, or by artificially introducing new-comers. But where the scientific man would try at least to act warily, the practical man is impetuous, and many illustrations of nemesis, *e.g.* the rabbits in Australia, are well-known. Nor has the scientific man always restrained himself from eliminating and introducing, and though the results have sometimes been beneficial, it has not always been so.

Apart from its practical importance, man's agency as an eliminator and distributor is of much theoretical interest, for the results serve to vivify our realisation of the struggle for existence, and often to impress us with the plasticity of adaptation which even highly specialised forms have still in reserve. It may be profitable, therefore, to bring together a few illustrations.

In 1850 the first house sparrows of Europe were introduced into America, and from that time to 1870, according to Merriam and Barrows (U.S. Department of Agriculture, Division of Economic Ornithology and Mammalogy, Bulletin I. 1889), upwards of 1500 are said to have been imported. They found themselves in conditions where the operation of natural selection was, in great measure, suspended as far as they were concerned. Commenting on this, Prof. Hermon C. Bumpus says (*Biol. Lectures Woods Holl*, Boston, 1898, pp. 1-15):—"They have found abundant food, convenient and safe nesting-places, practically no natural enemies, and unrivalled means of dispersal. Aside from an early and brief period of fostering care, they have been left to shift for themselves; natural agencies have since been at work, and in the relatively short space of forty years a continent has been not merely invaded, but inundated by an animal which, in its native habitat, has been fairly subservient to the regulations imposed by competing life." They may here and there recognise

their debt to man by destroying a few weeds, they may by their chirping cheer the heart of the simple, and they have enabled Mr. Bumpus to make an interesting study on variation, but on the whole they are a pest. Mr. Palmer, to whose article on dangerous introductions we shall immediately refer, says that the English bird "is now present in every state and territory, with half a dozen exceptions, and is known as a pest to nearly every one in the eastern United States. It has begun to spread in Argentina, while in Australia it is even more troublesome than in this country. It has also gained a foothold in Hawaii and numerous islands in the Atlantic, Pacific, and Indian Oceans." Most vigorous attempts have been made to get rid of it, *e.g.* the attempt last spring to expel it from Boston Common, but the sparrow holds its own. It is to be hoped that the proposal to introduce the English starling to counteract the English sparrow will not commend itself, for the evidence of antagonism seems very slim, and the cure might be worse than the disease.

In an article by T. S. Palmer, entitled "The Danger of introducing Noxious Animals and Birds," in the *Year-book of the Department of Agriculture* (U.S.A.) for 1898 (pp. 87-110, illustrated), of which the author gives an abstract in *Science* (x. 1899, pp. 174-176), some good examples will be found.

The mongoose, introduced into Jamaica in 1872 to keep down the rats, has multiplied like the rabbits in Australia and New Zealand, and while effectually reducing the rats, has proceeded to a wholesale destruction of poultry, game, ground-nesting birds of various kinds, reptiles, and even fruits. "The decrease of birds was followed by a marked increase in certain insect pests, but recent reports indicate that the mongoose is diminishing somewhat in numbers, and some of the birds are increasing, so that both native and introduced species are adapting themselves to new conditions." In Hawaii the record is similar, but the mongoose has not yet become such a nuisance as in Jamaica.

In the *Scientific American* for August 26, 1899, p. 140, Dr. C. M. Blackford recalls some other instances. In 1868 Leopold Trouvelot, an entomologist, was unfortunate enough to allow some imported gypsy moths (*Porthetria dispar*) to escape through an open window. In twenty years they had become a scourge, and we have more than once in our columns referred to their devastations and to the immense sums which have been expended in trying to counteract them. It is at last possible to say that the pest is under control, but the cost of its suppression has been enormous.

A happier instance of introduction is found in the well-known story of the fluted scale (*Icerya purchasi*) brought to California from Australia to the great damage of the orange and lemon groves, but effectively checked by the further introduction of the red "lady-bug" or vedalia (*Novius cardinalis*). "Within a short space of time the

trees were cleared, and at present the scales are being reared to preserve the lady-bugs in case of another outbreak." In 1897, in Portugal, the experience of the United States was successfully repeated.

From Dr. Blackford's article we may take one other example: "In many of the rivers of Brazil a plant grows that is called the Water Hyacinth. It is very ornamental, and a few years ago a land-owner on the St. John's River, in Florida, procured a small number for a pond on his estate. They increased rapidly and filled up the pond, whereupon the owner had them gathered up and thrown into the river. The experiment was unfortunate. Free from natural enemies, the hyacinths have flourished, so that on many streams navigation is practically impossible. From shore to shore there spreads an impenetrable sheet of vegetation that entangles paddles, oars, or propellers, and arrests all manner of refuse that should go to the sea. From time to time bodies of this growth become detached and drift down until salt water is reached, when the plants die and are cast ashore in putrescent heaps. A natural enemy has been sought, but as yet no appreciable result has been accomplished. In Brazil a small red spider lives on the hyacinths, and is said to be injurious to it. This spider has been introduced into Florida, but no effect has been perceived."

In conclusion, the theoretical interest of these cases is all very well, but "things are in the saddle," and practical considerations force themselves upon us. Therefore we have pleasure in quoting the last paragraph of Mr. Palmer's article. "Congress should take steps promptly to protect Hawaii and Puerto Rico against further introduction of noxious species, and to prevent the mongoose from being brought into the United States. The introduction of exotic mammals and birds should be restricted by law, and should be under the control of the U.S. Department of Agriculture. The wild rabbit, the mongoose, the flying foxes, and the mina of the Old World, should be rigidly excluded; and species of doubtful value, such as the starling, skylark, kohlmeise, and blackbird, should be imported with the greatest care, and only in places where they can be controlled in case they prove injurious."

Notes on American Mammals.

Mr. D. G. Elliot, so well known from his magnificent illustrated monographs of various groups of animals, as well as from his less pretentious handbooks of North American game and water birds, has recently turned his attention to faunistic work. The results of his labours have been presented to the public in the "Publications of the Field Columbian Museum," and comprise the mammalian fauna of the Olympic mountains, notes on certain reptiles and batrachians from the

same district, and descriptions of apparently new mammals from Oklahoma and Indian territory. Since the author is by no means addicted to unnecessary "splitting," it may be taken for granted that such forms as receive new names are certainly entitled to distinction.

To mention any of the smaller animals by name would be of no general interest; and we may therefore direct attention to his account of the Western Wapiti, which has only recently been brought to the notice of naturalists, although described long ago by Hamilton Smith under the name of *C. occidentalis.* Mr. Elliot regards it as merely a local variety of the Wapiti, and accordingly refers to it as *C. canadensis occidentalis.*

Failing to find any satisfactory characters in the antlers whereby it can be distinguished from the typical Eastern Wapiti, the author turns to the coloration of the animal, and writes as follows:—"In nearly all seasons of the year, except winter, the colour of the coat is apparently indistinguishable from that of the Rocky Mountain species, and I have seen a number of heads, killed in winter, that resembled precisely the Eastern animal, being in no wise any darker. But, as a rule, I believe in winter the head and neck of the Olympic Wapiti, together with the legs, reaching to the groin and rump, are black, varying in intensity and in a mixture of brown, among different individuals. This peculiar coloration I have never seen in the Eastern Wapiti, and when in this pelage the Olympic animal would be always readily recognisable. It is to be expected that all the animals inhabiting a country subjected to such an annual rainfall as in north-west Washington, would be very dark in appearance, and this is almost universally the case, all colours being intensified; and it is not surprising that the Wapiti should prove to be no exception to the rule, but assumes at certain seasons a partly black pelage. This colouring is practically the only character there is by which the Wapiti of the Olympics and Rocky Mountains can be separated, and when it is absent the animals are indistinguishable from each other."

In the geological series of the same journal (i. p. 181) Mr. E. S. Riggs describes certain Rodent remains from the Miocene of North America, which he refers to the hitherto imperfectly known family *Mylagaulidæ.* This family was established by the late Professor Cope on the evidence of jaws from the Upper Miocene of Nebraska described as *Mylagaulus.* The other forms, respectively from the Deep River and John Day beds, are named *Mesogaulus* and *Protogaulus.* Although showing some dental characters approximating to the Porcupines, these Rodents are regarded as undoubted Sciuromorphs, allied to the *Castoridæ,* although to a great extent forming an isolated type. "The one prominent feature," writes Mr. Riggs, "is the unusual development of the premolar, to the exclusion of the posterior-lying teeth. Associated with this is the great strength and sharpness of the mandible, the prominence and anterior position of the masseteric ridge, and the

depth of the ramus from the alveoli to the angle. These tell an unmistakable story;—unusual capacity for crushing or grinding, and the attendant specialisation of the premolar to perform the function laid upon it. Just as in the Carnivora, the first lower molar, lying immediately anterior to the insertion of the masseter muscles, has developed into the great shearing tooth; so in these forms the last premolar has fitted itself for a crushing implement, which has reached the highest degree of specialisation known to Rodentia." It is then suggested that the teeth in question may have been employed for cracking nuts or hard-shelled seeds, although evidently also used for grinding.

The 15th part of the "North American Fauna" is devoted to a monograph of the genus *Zapus* (jumping-mice), the range of which has recently been increased by the discovery of a species in North-West China. Mr. E. A. Preble is the author of the memoir in question, and appears to have done his work well.

The naturalists of the La Plata Museum appear convinced that the so-called *Neomylodon listai*—the ground-sloth, whose skin has been discovered in a cave in Patagonia—is really inseparable from the genus *Glossotherium*, or *Grypotherium*, and conclude that it was kept in a domestic state by the early inhabitants of Patagonia. They further believe it to be now extinct. The first instalment of a conjoint paper on the subject is published in the *Rev. Mus. La Plata*, vol. ix. p. 407.

American Plant-Notes.

Recent numbers of *Rhodora*, the journal of the New England Botanical Club, maintain the reputation of this small but useful periodical. Among numerous notes and short papers dealing chiefly with the native flora, we note some suggestions on seaweed collecting by F. S. Collins, and an account of past and present floral conditions in Central Massachusetts by G. E. Stone. The last mentioned traces the effects of deforestation on the flora, especially with regard to the proportion and nature of the trees. Several species, such as the hemlock, beech, and canoe-birch have become less abundant, their places being more or less occupied by the quicker growing white birch and poplar. The complete and continual removal of forest has also exerted a great influence upon many smaller plants, and there is a marked decline in the luxuriance of humus-loving orchids, strawberries and meadow-grasses.

The July number of the *Plant World* contains a picture and short account of the liberty tree of Annapolis, an ancient and magnificent tulip-tree with numerous and various historic associations. There is also a laudatory exposition of ecology, or the study of the relation of

plants to their surroundings as a branch of botany worthy the attention of teachers and students.

With the July number the *Botanical Gazette* enters on a new volume (xxviii). The issue contains three important papers. "Studies on Reduction in Plants," by G. F. Atkinson, describes the intra-nuclear changes occurring during pollen development in an aroid (*Arisaema triphyllum*), and a liliaceous plant (*Trillium grandiflorum*). The author suggests that "some of the bewilderment which now surrounds certain phases of the study of the morphology of the nucleus" will disappear, "if we recognise that there is such a thing as a reducing division or qualitative reduction in plants as represented by such types as *Trillium, Arisaema*," and others; "that there are plants in which only a quantitative or numerical reduction occurs," as in *Podophyllum;* "and possibly that there is still another type where in the same plant qualitative reduction may take place in some cells, while quantitative or numerical reduction only takes place in others." The paper is fully illustrated. Charles Robertson adds another (No. xix.) to his long list of papers on "Flowers and Insects." He deals chiefly with the flower visits of oligotropic bees, those, namely, which restrict their visits for pollen-collecting to a few flowers. Oligotropic species are more frequent than has hitherto been supposed, and the author gives a list of fifty-two belonging to thirteen genera, the number of plant species visited varying from one to nine. He also discusses the influence of bees in the modification of flowers, tracing the origin of pollination by insects, and the development of increasingly complex mechanism, as the result of insect-visits.

The "Origin of the Leafy Sporophyte" is a critical contribution by Prof. J. M. Coulter, to the much debated question of the development of the higher leaf-bearing plant from the moss-capsule or some one or more ancestors. The argument from cytology is not yet clear, and the author is fain to admit that, on the whole, all such discussion is "very vague and general, and may not commend itself to many as profitable."

We have received a separate copy, printed in advance from the eleventh annual report of the Missouri Botanical Garden, of a revision of the North American species of *Euphorbia*, belonging to the section *Tithymalus*. Most of our British spurges belong to this section, and nearly all of them have been introduced into the United States and have become more or less widely spread there. The paper, which is by J. B. S. Norton, is accompanied by no less than forty-two plates showing the general habit of the plant, with floral dissections and figures of the seeds. We have so often to deplore the absence of figures in systematic books and papers that we are glad to note an example of a monograph in which every species is figured.

A Rock out of Place.

In the July-August number of the *Journal of Geology* (vii. pp. 483-488) Stuart Weller describes the peculiar occurrence of a small patch of Upper Devonian rock in the heart of a quarry of Niagara limestone at Elmhurst, Illinois. At this locality the limestone is much fractured, and one of the joints is enlarged to form a cavity, triangular in section, 6 inches wide at the base and 16 inches high, but thinning out as it passes into the rock. This cavity is filled with angular fragments of the adjacent limestone embedded in a dark brown sandy matrix, which contains fish-teeth, *Lingula*, and other brachiopods, the total suggestive of a late Devonian age. From this material two new species of *Diplodus* are described by C. R. Eastman in the same number of the *Journal*. Further, says Dr. Weller, "At the base of the triangular opening, between the two beds of limestone that come in contact at that point, the Devonian material extends both to the right and left for several feet, forming a bed an inch or two in thickness between the two limestone beds."

The nearest outcrop of Devonian is 80 miles from Elmhurst, so that the position of this patch is doubly interesting. Dr. Weller explains it thus. During the greater part of Devonian time the region must have been above sea-level (an inference which seems to follow legitimately from the alleged age of the deposit). "The waters which collected upon this land surface in part percolated through the underlying rock strata and by solution increased the size of many joint cracks. At a later period, near the close of the Devonian, when the sea again occupied the region, sand was sifted down into these open joints, and with it the teeth of fishes which inhabited the sea thereabout." The opening was, "perhaps, large enough for the entrance of some of these fishes." Traces of the same sandy material are seen on the joint-face above the opening.

If this explanation be true, then, as Dr. Weller phrases it, "no description of any similar occurrence has been observed in the literature"; but this scarcely justifies the conclusion of the sentence, "and it may be designated by the name subterranean unconformity." If the mode of occurrence were at all common, if it were anything but unique, then perhaps a name might be convenient. At present there seems no advantage in one. Moreover, we are not convinced that Dr. Weller's account is the true one. It does not allude to the occurrence of clay in the other joints, and it affords no explanation of the limestone breccia. Can Dr. Weller prove that this is not a fault-rock, in which fragments of the immediately adjacent rock are mixed up with fragments or washings that have fallen down the crack from the superjacent rock? Such an occurrence is common enough, though we know no technical name for it.

Beeren Eiland.

THE Swedish Arctic Expedition of 1898, under the leadership of Prof. A. G. Nathorst, spent a week on Beeren Eiland, mapped it on a scale of 1 : 50,000, and made numerous observations on its natural history. Chief among these were the geological researches which proved a prehistoric local glaciation, and by means of fossils showed the presence of rocks of three systems: Silurian, Middle Carboniferous, and Trias, previously unknown on the island. These discoveries led to another expedition to Beeren Eiland during the past summer. The expenses were borne by the Vega Stipend of the Swedish Geographical Society, the Lars Hierta Memorial Fund, and various private individuals. The leader was the geologist, J. Gunnar Andersson of Upsala, who had accompanied Prof. Nathorst; the other scientific members were C. A. Forsberg, cartographer and meteorologist, and G. Swenander, zoologist and botanist. The expedition stayed on Beeren Eiland from June 23 to August 19, and accomplished the following work:—

The whole island was mapped in greater detail, and a special map, on a scale of 1 : 5000, was made of Rysshamn, where the expedition had its headquarters.

From June 25 to August 16 complete meteorological observations were taken twice a day, as well as continuous observations by a self-registering barometer and thermometer. Eight series of observations were made on the tides, each series extending over from 8 to 51 hours, during which time the height of the water at intervals of half an hour was marked off on a section.

The botanist collected all the phanerogams previously found on the island as well as *Koenigia islandica,* hitherto unrecorded. Exhaustive collections were also made of the lower plants, including the algae of red and green snow. To investigate the influence on plant-growth of the continuous light of an Arctic summer, three series of cultivation experiments were carried out as follows:—First, in five places of nearly the same longitude, but at a distance of about 3 or 4 degrees of latitude from one another—namely, Svalöf in Scania, Ultuna near Upsala, Luleå, Tromsö, and Beeren Eiland—barley taken from the same sample was grown in soil from the same place. Only the climatic conditions, and especially those of light, were different in the different stations; thus there were completely dark nights in Scania, complete light the whole 24 hours on Beeren Eiland, with intermediate conditions at the intervening places. The material from the Scandinavian stations has not yet been brought in, so that the results of this interesting experiment are still awaited. Secondly, on open land at the Beeren Eiland station there were cultivated two precisely similar series of Arctic plants, of which one series stood in continual light, while the other was kept in complete darkness each night (8 P.M. to

8 A.M.). During the period of the experiment the development of these plants did not proceed very far, but the series kept in the light was obviously the more sturdy. The third experiment consisted in the cultivation, on a hot-bed, of a score of common Scandinavian plants. These also were in two similar series, one kept in the light, the other darkened by night. The experiment succeeded with 18, and of these 16 were clearly more sturdy in the light series, some of them yielding examples half as large again as those in the darkened series.

To the list of the island's fauna were added two birds: the Skua (*Lestris pomatorhina*) and the Spitzbergen form of *Mormon arcticus*. *Salmo alpinus* was found in a lake. Special attention was paid to the insects, which on isolated oceanic islands are of much interest to the student of distribution. Holmgren, the only entomologist who had previously visited Beeren Eiland, found there in 1868 only 9 species of Diptera and 1 Hymenopteron. The Swedish expedition has brought back a large collection of Diptera, not yet worked through, 4 Hymenoptera, 1 Neuropteron, and 2 Coleoptera. Holmgren found only 2 Acarids; the present explorers have at least 10.

The chief object of the expedition was a detailed geological investigation of the island. This has been successfully carried out with valuable results. A large collection of fossil plants from the coal-bearing series has been made; numerous fossils have been collected from all the marine strata, especially from the Trias. A geological map of the whole island has been constructed. The stratigraphy and tectonic geology of the whole island has been worked out, and there have been discovered in the southern part of the island a series of dislocations of Carboniferous age, which explain the topography of the hilly regions and the varying development of the Carboniferous system at various points.

Mr. Gunnar Andersson and his companions are to be congratulated on the amount of solid work they have accomplished, and we look forward to the publication of the detailed results with much interest. It should be mentioned that the proprietor of Beeren Eiland, Mr. Lerner (who happens to be a German) has helped the expedition, and hopes to welcome it back in some future year.

The Difficulties of the Australian Museum.

DESPITE the fact that the Australian Museum is in an unhappy financial position, we enjoy reading the report of its Curator, because Mr. R. Etheridge, junior, has a way of saying just what he thinks, and this way—the essence of all great literature—is not permitted to many officials in the mother country. Mr. Etheridge's vigour has

infected even the Trustees, and their Report for the year 1898 puts the case as strongly as can be expected from so decorous a body. They "regret that for some years past the funds voted for the maintenance of the museum have been inadequate. In 1892 the museum vote, leaving out of account special items, was £7201. In 1893 the trustees were compelled to submit to considerable reductions, rendered necessary by the financial pressure of the time, and they endeavoured to adapt their work to the rates allowed. They expected, however, that with returning prosperity, not only would former votes have been restored, but that some material consideration would have been given to the natural advancement of the institution." This has not been the case, since the appropriation for 1898-99, although showing slight increase, was over £2000 less than that for 1892. "As regards members of the scientific staff, no steps have been taken towards restoring the salaries to the rates existing before the retrenchment of 1893, although, in the public service generally, considerable increases have been granted to officers. In 1892 the vote for purchases was £1250; since 1893 only £200 a year have been allowed, including purchase of books as well as specimens." Such a sum would be ridiculously small for a metropolitan museum, if assigned to books alone. "Consequently, many desirable specimens have been lost to the Museum, and therefore to the Colony, while no collecting, so necessary for maintenance as well as increase of the exhibits, has been done, and the Library has also fallen into arrears. The insufficiency of the funds provided for the Museum by the statutory endowment of £1000 per annum, together with the irregularity both in amounts and in detail of the Annual Votes of Parliament, supplementary to the endowment, prevent anything like an effective promotion of the interests of science in connection with the natural history of the Colony. As those interests have an important relationship to the development of the resources, and, consequently, to the future prosperity of the community, the Trustees are exceedingly anxious to be placed in a better position for carrying out the purposes for which the Museum has been established."

With all this, needless to say, we heartily sympathise. We do not overlook the fact that last year a sum of £1500 was placed on the Estimates for certain much-needed repairs, or that on the Loan Estimates for 1898-99 a further sum of £13,500 has been voted for museum extension, the intention being to build the superstructure over the newly-erected workshops as a portion of the south wing. But we observe that "very great and unnecessary delay has arisen in the carrying out of the renovations," and we emphasise the contention of Mr. Etheridge that, as the collections and the buildings grow, it is necessary to increase the staff, and to provide at least sufficient money for cases and for locks to them. The admirable work carried out by this excellently-administered museum has often been alluded to by us,

and further information regarding it will be found in our news-pages. It would be a serious loss to the colony should the activities of the staff continue to be restricted, and should the valuable collections suffer yet further neglect.

The *Antarctic* in the Arctic.

THE Swedish expedition to the coast of East Greenland, under the leadership of Professor A. G. Nathorst, on board the ss. *Antarctic* (Captain Forssell), returned to Stockholm in September, having accomplished some excellent work. The ice at first was found to be heavy, so some time was spent in exploring Jan Mayen Island. As soon as the ice permitted, an advance was made in the direction of Shannon Island; but here again the ice prevented a passage from being forced, and the *Antarctic* steamed south to Scoresby Sound. Various observations and corrections of the chart were made here, Hurry Inlet being found closed to the north. The expedition then returned north, and this time succeeded in entering Franz Josef Fjord. This was found to extend very much less into the interior than shown on Payer's chart, and Petermann's Peak also was found to have about half the height assigned to it by Payer. To make up for this, the expedition discovered a new fjord system, with three branches, stretching south from the mouth of Franz Josef Fjord, to a distance equalling that of the great Sogne Fjord in Norway. To this Professor Nathorst has given the name Kung Oscar Fjord. Eight weeks were spent in investigating its shores and those of Franz Josef Fjord, and a map of them was made on the scale of 1 : 200,000. Among the interesting discoveries reported by Professor Nathorst is that of Devonian rocks with armoured fish. Silurian fossils also have been found. Several individuals of that curious animal, the musk-ox, were seen and shot. The flesh was found to have a muttony flavour with no unpleasant scent, and Professor Nathorst suggests the acclimatisation of the animal in northern Sweden. Polar bears and a few Arctic foxes also were seen by members of the expedition. Large collections of marine animals were made and are now being worked up in the Riksmuseum at Stockholm. Among the notable specimens is one of the pennatulid, *Umbellularia*, with a stem over six feet long. It was only in the accomplishment of its ostensible object, the finding of some trace of Andrée, that the expedition failed. Since the Royal Geographical Society contributed £100 to the expense, we shall doubtless be able to read further details in its *Journal*.

Regeneration in Orthoptera.

Mr. Edmund Bordage, of Réunion, although recently laid aside by fever, continues to send home notes in regard to regeneration in Phasmidae, Mantidae, Blattidae, and other Orthoptera. Their theoretical interest is so great that we venture to refer at some length to two or three recent papers by this observer.

In twenty-five species of Orthoptera with five-jointed tarsus, representing twenty-one genera and three families, the regenerated tarsus has only four joints. The number given in his published paper is eighteen species, but a manuscript note on the copy sent us states it at twenty-five.

In *Phylloptera laurifolia* and *Conoecphalus differens* (Locustidae), *Acridium rubellum* (Acrididae), and *Gryllus campestris* (Gryllidae), there is no trace of regenerative capacity in connection with the posterior legs, which are used in jumping. This appears at first sight an argument against the generality of Lessona's law, since these hind legs are surely much exposed to the bites of enemies, besides being liable to injury in the moults. Bordage's answer is that the loss of these limbs makes moulting extremely difficult, exposes the insects to great danger at the hands of their enemies, prevents copulation, and places the unfortunates at a great disadvantage in preferential mating. He concludes that jumping Orthoptera which have lost their hindmost legs are unable to propagate, and that this explains the absence of regenerative capacity in this particular case.

In another paper (*Comptes Rendus Acad. Sci. Paris,* cxxix. 1899, pp. 169-171), Bordage points out that it is impossible to provoke autotomy of the first two pairs of legs in saltatorial Orthoptera. By main force a separation may be effected at the articulation of trochanter and coxa, or rarely at the articulation of femur and trochanter. The mutilation is often fatal, but if the insect survives and is still larval, regeneration may be effected, perfectly if the separation was between femur and trochanter, more or less rudimentarily if between trochanter and coxa.

This raises a double difficulty for those who uphold Lessona's law: —(1) the regeneration seems to occur at points where mutilation cannot be naturally effected; and (2) the regeneration is most frequent and most complete when the separation has been effected along the line where rupture is rarest.

Bordage gets over the difficulty by pointing out that in the "exuvial autotomy," *i.e.* self-mutilation during a moult, the separation is most frequent along the femur-trochanter articulation, and very rare along the trochanter-coxa articulation. The bleeding is insignificant in the first case, but it may be fatal in the second. Moreover, regeneration in the first case is frequent, and, though slow, sometimes

perfect; but in the second case an unjointed stump is formed. There seems to be no appendage which may not suffer mutilation during the hazardous process of moulting.

In jumping Orthoptera, tarsal regeneration occurs readily on any of the legs, and this conforms with the fact that mutilation of the tarsus is peculiarly liable to occur during moulting. Experiment shows that the terminal portion of the tibia may also be regenerated, and this too may be associated with the fact that in exuvial mutilation or, more rarely, as the result of attack, the muscles at the end of the tibia are often torn when the tarsus is pulled off.

Bordage also notes that in *Phylloptera laurifolia* and *Conocephalus differens* the regenerated tarsus is tetrameral, as is normal in Locustidae, while in *Gryllus campestris* the regenerated tarsus has three joints. In Locustidae and Gryllidae the tibia of regenerated anterior legs does not possess the tympanic apparatus borne on the normal limb.

Diastataxy.

THE *Journal of the Linnean Society*—Zoology—for July, vol. xxvii., contains two very important contributions towards a solution of that ornithological puzzle known hitherto as "Aquinto-cubitalism." Mr. P. Chalmers Mitchell has approached the question from the point of view of comparative anatomy; Mr. W. P. Pycraft from that of embryology.

The riddle to be solved, it will be remembered, was the meaning of the constant absence of a remex from between the fifth pair of secondary major coverts of the wing in certain birds, or groups of birds. Wings in which this feather was wanting were known as aquinto-cubital; when there was no such deficiency the wing was known as "quinto-cubital."

Mr. Mitchell has proposed the term diastataxic for the former, and eutaxic for the latter. These terms are undoubtedly superior to the older ones, and have been adopted by Mr. Pycraft in his paper.

Till now, it was believed that in the diastataxic wing the fifth remex was missing; both the present authors agree, however, that this is not the case.

Mr. Pycraft endeavours to show that the remex in question has lost its original relations, but not its existence. According to him the diastataxial wing is at first eutaxic, changing more or less suddenly during development from the one into the other. This is brought about by a remarkable, but unmistakable shifting of position of all the coverts of the dorsal surface of the wing and of the remiges (1-4). The remiges in question move outwards (wrist-wards), and backwards, the movement being accompanied by certain of the obliquely transverse row of coverts (1-5). As a consequence, the fifth of these rows becomes

separated from its remex (the fifth), and comes to lie, in the adult, quill-less between quills—the fourth and fifth. The place of the fifth oblique row of coverts is now taken by the sixth which runs outwards, that of the sixth by the seventh, and so on inwards to the elbow, thus, each obliquely transverse row from the wrist inwards moves forward one place, as also do the remiges (1-4); the remainder appear to be stationary. Thus does the eutaxic wing become diastataxic.

Numerous figures of embryonic wings leave little doubt that this interpretation of the mystery is correct; what we want to know is "Why this shifting?"

Mr. Mitchell, in the pterylogical section of his paper claims to have proved that the diastataxic wing is architaxial, and not the eutaxic, as is held by Mr. Pycraft. He endeavours to support his claim by demonstrating the transition from diastataxy to eutaxy in the wings of certain pigeons—upon which group the whole of his observations are based.

This transition seems to be brought about by a shortening of the wing, and the obliteration of the usually more or less marked gap between the fourth and fifth remiges. But more than this; the crux of his paper rests upon the identity of certain covert feathers lying in the interspace between the fourth and fifth remiges just referred to. In certain of the pigeons he has examined, as in many other birds, the median coverts lie in the interspaces between the remiges. As a consequence, in the diastema between remiges 4-5 we have a major covert lying between two median coverts. In some pigeons two of these three feathers disappear, and the wing, according to Mr. Mitchell, becomes eutaxic. Assuming that the remaining covert is of the median series he has proved his contention. If, however, as he himself suggests, it is a major covert, his contention is only partly true. We have a pseudo-eutaxy. This last point is one of very considerable importance, for it may happen that, after all, the apparently eutaxic forms which occur amongst diastataxic groups may prove ultimately to be pseudo-eutaxic. At least this must be so, if we define diastataxy as that form of wing lacking a secondary remex from between the fifth pair of major coverts. This interpretation would be quite in harmony with Mr. Pycraft's paper. It is to be hoped that further research will be made in this part of the subject.

Immunity acquired before Birth.

It is of interest to students of heredity to note the observations of Messrs. Béclère, Chambon, Ménard, and Coulomb (*Comptes Rendus Acad. Sci. Paris*, cxxix. 1899, pp. 235-37), on sixty-five mothers and an equal number of newly-born children. The results make it

difficult to deny the justness of the interpretation that in certain cases there is a passage of antivirulent substance from the blood of a vaccinally-immune pregnant mother to the blood of the foetus, and that the child may be in consequence born immune.

The facts and arguments are briefly the following:—Immunity to vaccinal inoculation was observed only in children whose mothers were immune. Only in a small number of cases where the mother was immune was the child immune. The intra-uterine transmission occurred in cases where the maternal serum was antivirulent, irrespective of the period when the mother was vaccinated. On the other hand, the intra-uterine transmission was not observed in any case where the maternal serum was non-antivirulent, although vaccination had been effected shortly before or during pregnancy. Therefore the condition of so-called congenital immunity is the transmission of antivirulent substance from the maternal to the foetal blood through the placenta. But the condition may be fulfilled without result, for some of the newly born, whose serum was antivirulent, were still inoculated with success. In fact, the degree of antivirulent potency is variable, but it may be said that the more antivirulent the serum shows itself to be, the greater is the presumption that the vaccinal inoculation will fail to have effect.

Eel Poison and Cellular Immunity.

THE serum of eels is known to contain a "globulicidal" substance—ichthyotoxin, of course—which destroys the red blood-corpuscles of various animals into which it has been injected. The rabbit, for instance, is peculiarly susceptible; the red blood-corpuscles rapidly lose their haemoglobin by diffusion when the eel-serum is injected, even when it is diluted to $\frac{1}{10000}$ — $\frac{1}{20000}$.

Messrs. L. Camus and E. Gley, who have investigated the subject (*Comptes Rendus Acad. Sci. Paris*, cxxix. 1899, pp. 231-233), find that the hedgehog is very immune, even against strong injections, and experiments show that this immunity is due, not to the presence of any "antiglobulicide" in the hedgehog's serum, but to the resistent power of the red blood-corpuscles themselves. They have a natural cellular immunity, wrongly called by the authors "immunité *cytologique*." (If words mean anything it should be *cellulaire*, but the mistake is a common one.)

The frog and the toad, the hen and the pigeon, and *Vespertilio murinus*, show the same cellular immunity. A peculiar fact is that newly born, still blind rabbits, are similarly resistent, but the power dwindles from the fifteenth day or so, and the adults have none. The experimenters, in their interesting paper, cite the case of a doe-rabbit

which was rendered immune, and had thereafter young ones. These proved to have cellular immunity, but the presence of an antiglobulicidal substance was detected in their serum, so that both natural cellular immunity and acquired "humoral" immunity were found co-existing in one organism. What next?

Venom of Vipers.

MANY wonderful things have been discovered of recent years in regard to the poison of snakes, such as the possibility of counteracting its toxic properties with the snake's own bile, but that there is still much to be discovered is evident from a recent communication from a well-known worker, C. Phisalix (*Comptes Rendus Acad. Sci. Paris*, cxxix. 1899, pp. 115-17).

He has shown that the secretion of the poison glands of *Vipera aspis* and other Viperidae contains a diastatic ferment or echidnase. This varies in amount according to habitat and season. Thus it is much more abundant in vipers of the Vendée than in those of Arbois (Jura); it is not demonstrable in the secretion in early spring after the hibernal period, but has become abundant by the end of May or the beginning of June. It is indeed present in the glands in spring, but the secretory cells are inactive and retain it.

A solution of the viper's poison in glycerine-water gradually loses its virulence, more quickly when the external temperature is high. It often happens that in ten to fifteen days the venom has become quite innocuous, and it is found that of its active principles the echidnase is most persistent. Moreover, when the echidnase is removed from the venom, the attenuation of the latter is much slower than usual. It is therefore logical to suppose that the echidnase plays an active part in the attenuation, directly attacking the venomous principle. Experiments show that this is really the case, and thus we reach the conclusion that the diastatic ferment of Viperidae has a digestive effect not only on the tissues of the animals inoculated, but also on the active toxic substance, the echidno-toxin.

ORIGINAL COMMUNICATIONS.

Variation-Statistics in Zoology.[1]

By Dr. Georg Duncker.

Zoological and botanical objects have usually been regarded as isolated products of nature, to be described carefully and to be grouped, according to the degree of their morphological and ontogenetic likeness, under abstract conceptions, the systematic categories. Yet this customary manner of considering biological objects is insufficient, since individuals of any systematic category never occur isolated, but always in larger or smaller complexes or groups of individuals. For about ten years another point of view has been finding increased favour; it has been recognised that not only the morphological characters of single individuals, but especially those of the natural complexes of homogeneous individuals, are worthy objects of investigation. This kind of investigation takes the same place in zoology and botany as ethnography does in anthropology. In reference to its special aims, it has worked out its own methods, which in its various stages of development and different branches, may be summed up under the title *Statistics of Variation*. Let us briefly consider this method and its results.

In anthropology statistics of variation have been already utilised for forty or fifty years. This is partly due to the early scientific interest in the individual differences of the characters of man, and partly to the fact that the problems of this branch of biology cannot be solved by isolated observations made on single individuals, but require intensive investigations of the actual groups of individuals. This is evidently the case with racial problems in anthropology.

Primarily, zoology and botany are occupied in investigating the characters and development of individuals (anatomical morphology and embryology). We also find that the individuals, morphologically dissimilar, are classifiable, according to their degree of likeness, into higher or lower categories, which are regarded as elementary objects of scientific investigation (systematic zoology, comparative anatomy).

[1] Lecture delivered at the meeting of the Deutsche Zoologische Gesellschaft at Hamburg, May 1899.

That elementary complex of individuals, which is usually the starting-point for zoological and botanical investigations, is the species. More or less exclusively, all biological, systematic, and anatomical results are referred to this. But the species is by no means an elementary group; even if we omit its systematic sub-groups, the variety and the race, we find it empirically to be composed of individuals which are separated by space and time, and are allied to each other in different degrees of kinship. In those individuals of the same species there regularly occur morphological differentiations of their common characters, caused by constitutional factors (conditions of sex, stage of development) as well as by the sum total of recognisable external conditions of life (locality, geological formation, climate, food, etc.). Really elementary complexes of individuals, *eo ipso* coherent, are only those of which the morphological qualities have not been differentiated by any of the factors just enumerated. But even in such a "FORM-UNIT," as I have called it [7], we find on investigating the distinctive characters that there are individual differences.

Therefore the species is not elemental, a conclusion strengthened by the difficulty (bordering upon impossibility) of definition. It splits up in numerous variable form-units, produced by different factors, which frequently may be united into races or varieties. Each form-unit is a sum of more or less different individuals, the characters of which change in the course of development, that is, in time, but appear constant at a given moment, so that it is not justifiable to speak of varying or non-varying individuals. On the other hand, groups of individuals are variable in every moment of their existence and in each of their characters. Therefore *the fact of variation is to be seen only in the characters of groups of individuals,* and to be investigated only in these.

The exact study of variation affords a better understanding of the systematic relations between groups of individuals; it is a means of distinguishing pathological from normal states; but it owes its highest importance to its bearing on the theoretical explanation of the relations between organic individuals, *i.e.* in regard to heredity and evolution.

The objects of an investigation on variability are the characters of a complex of individuals and, according to the laws of induction, at first of the most primitive complex, the form-unit. The aim of this investigation is twofold; on the one hand qualitative, to discover the real individual differences in these characters, which we may call the *variants;* on the other, quantitative, to discover the relative frequencies of the single variants of each character determined.

The principal difference between a character of a single individual and one of a complex of individuals, therefore, consists in the possibility of expressing the former by a single variant, while the latter requires

not only several variants, but also their relative frequencies. Until now this necessity has generally been neglected. The characters of a group of individuals, *e.g.* of the species, have been described by uncritical generalisation of single results which were regarded as "typical" or "normal," or by average values got mostly from small numbers of observations, which naturally represent only idealised single results, or, in the best cases, by so-called "ranges of variation." The latter are merely chance results of observation without definitive value; they show the group to be variable, without indicating the manner of its variation. The only quantitative data we occasionally meet with are indefinite terms, such as "frequent" or "rare."

For determining not only the variants of comparable objects, but their frequencies as well, we must use statistics. Statistics are collections of single data, brought together according to certain points of view, of qualitative differences of numerous objects belonging to the complex to be investigated, and of the frequencies with which these differences occur.

In order to investigate the variation of any character of a form-unit, the character in question has to be determined in as many individuals of the form-unit as possible, the variants in which the character is found are to be noted, and finally the frequency with which each of these variants occurs, is to be determined. This method can be applied to every character, to conditions of shape and colour as well as to dimensional or numerical conditions of organs.

Then the first result as regards variation will be, that if the number of individuals investigated is not too small, the relative frequencies of the single variants of the character will be nearly constant in each lot of the same form-unit. For instance, when a character has been investigated thrice, each time in 500 individuals, and in all cases nearly equal percentages of its variants have been found, we may conclude, according to the law of great numbers, that in the whole form-unit also the variants are distributed in the same proportion. Secondly, closely allied form-units, *e.g.* the two sexes of the same breed and in a similar stage of development, may possibly agree in the mean and range of a character, and yet sensibly differ in the frequency distribution of its variants. Such differences of complexes of individuals are only to be made out by statistically examining the variability of their characters.

The statistical investigation of such characters as cannot be numerically expressed, like conditions of shape and of colour,[1] cannot go beyond this point. But in numerical characters, such as dimensions or numbers of homologous organs, the variants represent numbers which differ by constant values, the units of dimension or enumera-

[1] At present there is an increasing tendency to express numerically these conditions also; thus Davenport seeks to numerically determine colour-variations by the "colour-wheel" (*Science*, N.S. vol. ix. No. 220, p. 415-416).

tion. This done, the results may be further dealt with. Firstly, all the observed variants have to be arranged in series according to their numerical value, and the frequency of each among the (n) investigated individuals has to be determined. Thus we get the *empirical series of variation* of the character in n individuals. Weldon [20], for example, counted the number of the dorsal rostral teeth in 915 individuals of *Palaemonetes varians,* whence he got

Variants .	1	2	3	4	5	6	7 (rostral teeth)
Frequencies .	2	18	123	372	349	50	1 (individuals).

A series of variation may be graphically represented by noting the variants in the order of their numerical values as points of equal distances on an abscissa, and by erecting ordinates from each of these points which represent by their length the relative (percentage) frequencies of the corresponding variants. Straight lines drawn between the free ends of each two adjacent ordinates, together with the abscissa, will give the outline of the *polygon of variation* of the character. The average value of the character, that is, the arithmetical mean of the variants, corresponds to a point on the abscissa (M); the ordinate erected to the latter is the *centroid vertical* of the polygon (y_c). The summit of the polygon of variation usually lies near the centroid vertical; its variant has been called the *mode*; but the mode is neither more "typical" nor more "normal" than any other variant existing.

The single ordinates of frequency are generally lower the more distant they are from the centroid vertical. The polygon of variation is broad and low when there is great variability in the character, but high and narrow in the opposite case. The best and simplest expression of the degree of variability of a given character is the square root of the average square deviations of its variants from the mean. This value, which may be called the *index of variability* of the character (ϵ), corresponds to a piece of the abscissa; it is expressed by the same unit as the variants of the character. The above-cited series of variation of *Palaemonetes* has the index of variability, ·8627 rostral teeth.

While the average values of a character may differ widely in different form-units of the same species, the indices of variability remain fairly constant [1] not only in the form-units of the same species,

[1] Examples:

I. Number of fin rays in	dorsal fin M	dorsal fin ϵ	anal fin M	anal fin ϵ
of *Pleuronectes flesus,* Baltic			39·46	1·4838
,, ,, ,, North Sea			41·56	1·7739
,, ,, ,, Plymouth	61·7214	2·3895	43·6098	1·6026
,, ,, *americanus* (BUMPUS [4])	65·06	2·4467	48·62	1·8188
,, *Rhombus maximus* (PETERSEN [14])	62·98	2·2533	45·86	1·6792
II. Number of rostral teeth	dorsal		ventral	
in *Palaemonetes varians* (WELDON [20])	4·3137	0·8627	1·6948	0·4799
,, ,, *vulgaris*	8·2819	0·8145	2·9781	0·4477

but also in those of species belonging to different genera, even to different families. This fact does not seem to me to have been sufficiently regarded hitherto; the explanation of it is, I suppose, the constancy of the physiological capacity of a given organ for reacting to the individual causes of variation (to be considered afterwards) with respect to a given character. Some authors, however, seem to assume a more or less constant relation between the height of the average and that of the index of variability of a character.

Average value and index of variability of a numerical character are the first data necessary to the description of its variation. Both ought always to be determined; but they only give an approximate idea of the variation of the character. Its complete description requires the determination of the curve which rules the slope of its polygon of variation, or in other words, on which the corner points of the polygon are situated. To find this curve, we must find the mathematical relations between the variants or their deviations from the average value on one hand and their frequencies on the other.

There is a striking likeness, even at the first glance, between the polygons of variation and binomial polygons. We get the latter by graphically representing the series of summation which arise by developing binomial terms, as $(p+q)^t$. As a matter of fact, both are closely related. In numerical characters we find variants deviating from the average value in positive and in negative directions. Since all processes in nature depend upon causes, we are obliged to assume causes of variation with either positive or negative effects, of which causes neither the number nor the intensity of effect is known. These causes must be different from those which determine the average character of the form-unit, and at the same time must be weaker in effect than the latter. Now each individual has its own fate, which word includes the total sum of enormously numerous and minute factors acting on it in the most diverse combinations, which naturally cannot be identical either for all individuals of the form-unit or in every moment of the existence of the single individual. Thus we get the conception of an enormous number of elementary causes of variation, which may be regarded as equally effective so far as their small power goes. Of these one set can effect positive deviations, the other negative deviations from the average values of the different characters, all being able to act on each individual of the form-unit, though as a matter of fact they do not all act on it. The active set of causes is in each case any random combination of positively and negatively acting causes, and each of these combinations has a higher or lower degree of probability, according to which its effect is more or less frequent in the total population of individuals. The sum total of positively acting causes may be equal in number to that of negatively acting ones, or be different from it.

Starting from such assumptions, mathematicians have investigated

the series of variation of numerical characters, and have found that the actual magnitude of the frequencies of variants corresponds to the law of probability of combinations, which law Pearson [12] has recently expressed by his *general probability curve* (*curve of variation*). This is, as far as I know, the first substantiated mathematical law of biological processes. So in investigating a series of variations we have next to find the probability curve determining the shape of its polygon of variation. But this demands a consideration of the already somewhat compendious mathematical literature of the subject, which I cannot now discuss. Pearson's methods I have recently described in a manner especially suited to the needs of biologists [7].

Curves of variation are symmetrical if the two groups of causes of variation are equal in number; asymmetrical, if the latter are unequal; in the single form-unit they always show one summit. In symmetrical curves the maximal ordinate and the centroid vertical are identical; in asymmetrical curves their distance apart is greater the more asymmetrical the curve. The ratio between this distance and the index of variability gives an abstract number, the *index of asymmetry* of the curve (A), which is, corresponding to the position of the centroid vertical to the maximal ordinate, either positive or negative. Positive asymmetry of a curve means that there are more negative than positive causes of variation, while negative asymmetry implies the contrary.

The question as to the variation of a numerical character within a form-unit is therefore to be answered by giving the average value, the indices of variability and of asymmetry, and the formula of the curve of variation of this character. These four data given, the series of variation can always be reconstructed with only a small error, which decreases as the number of investigated individuals is increased.[1] The first three data of our example (*Palaemonetes*) are:—

$$\mathrm{M} = 4{\cdot}3137, \quad \epsilon = {\cdot}8627, \quad = \mathrm{A} - {\cdot}1735\,;$$

the curve itself is a curve of type iv. (Pearson [12]) of the form

$$y = y_0(\cos\,\theta)^{2m\,\frac{-}{l}\,\tau\theta}$$

where y_0, m, and τ are constants, $\theta = f\,(x)$ the variable. The error between the empirical and the theoretical series of variation amounts to only ·3% of the total number of individuals, viz.:—

Variants .	0	1	2	3	4	5	6	7	rostral teeth).
Emp. frequency .	0	2	18	123	372	349	50	1	(individuals).
Theor. frequency (y)	·1	1·7	18·3	122·2	374·6	345·9	51·7	·5	

From the curve of variation the probable range of variation of the

[1] The magnitude of the error is, *ceteris paribus*, inversely proportional to the square root of the number of investigated individuals.

character may be deduced for any assumed number of individuals; since a curve of variation is a curve of probability, the range depends really upon this number; for instance, a variant of the probability $\frac{1}{10000}$ is hardly to be expected among only 100 individuals. On the contrary, if we find with an otherwise harmonious curve of variation some single extreme variant empirically more abundant than it ought to be according to its probability, we may conclude that this variant did not arise by normal variation, or at least not exclusively by it, but that it has been produced by pathological conditions. This conclusion is to be controlled by determination of the correlation-coefficients which we shall discuss later on. Thus my attention was directed to the hitherto apparently unknown ability of Syngnathidae which have lost the posterior segments of the body, to regenerate not only a complete caudal fin, but probably also a urostyle. I shall refer elsewhere to these observations and to some experiments confirming them.

Comparing several form-units of the same, or of different species, as to a single numerical character, all possible differences of the latter must clearly be recognised in the differences of its four statistical data. Having investigated all the form-units of a species in respect to a single character, we should find by graphically representing the results a system of curves of variation partly overlapping, of which the centroid verticals would be more or less distant from each other, while the indices of variability would be nearly constant. One set of the form-units represents the constitutional differences of the species due to sex and degree of development, the others correspond to differences in the external conditions. If the latter conditions have influenced not only one character but several characters at once, the species has been split up into races or varieties.

Up to this point we have dealt only with variation of a single character within the form-unit. But since all the characters vary, we must investigate whether they vary independently of each other, or whether there is possibly any relation between the variations of different characters. Here also we have recourse to calculating probabilities. Every one knows that the probability of the coincidence of several events independent of each other equals the product of the probabilities of each single event. From each deviation from this condition within a larger series of observations we may conclude the existence of some causal relation between the events, that is in our case between the individually combined variants. This causal relation can be a direct one, if the variants of one character are causes of the other (correlation *sensu stricto*) or an indirect one, if both characters depend upon the same causes of variation (symplasy). Thus, by simply comparing the real with the probable frequencies of the individual combinations of the variants of two characters within a larger number of individuals of the same form-unit, we are always

able to find out whether there is *correlation* between these characters or not.

For numerical characters there are now simple methods (Galton, Pearson) of calculating the *degree* of deviation of the real frequencies of the combinations of their variants from the probable ones. The results of these calculations are abstract numbers between zero (no deviation from probability) and one; the latter signifies the highest possible degree of deviation of the combination-frequencies from probability, inasmuch as each variant of the one character occurs only combined with a definite single variant of the other. These abstract numbers we call the *coefficients of correlation* of the investigated pairs of characters. The most convenient coefficient of correlation is that calculated according to Pearson's method [13], and determined as the mean product of the individually combined relative deviations of the two characters from their average values, while the relative deviations are the absolute ones expressed in terms of their indices of variability. Like the indices of variability of homologous characters, the coefficients of correlation of homologous pairs of characters show a certain constancy even in different species (Warren [17]). This, again, I believe to be an expression of physiological relations between the correlated organs with regard to the respective characters.

If on the average the combined variants lie either, on the one hand, both above or both below the mean values of the two characters, or, on the other hand, if the one is a positive, the other a negative deviation from these values, we get either a positive or a negative coefficient of correlation, and accordingly deal with positive or negative correlation. Series of variation between which there is positive correlation tend to form constant differences of the individually combined variants, while those between which there is negative correlation tend to form constant sums of the variants. The constancy of these sums or differences is the more remarkable, the higher the coefficient of correlation. The constancy of the sums of variants, that is, negative correlation, is mostly to be found in metamerically disposed characters (homoiotic variation), that of the differences of variants, that is, positive correlation, in antimerically disposed characters, especially in those with symmetrical variation.

As it is possible to investigate the probability or the degree of correlation of the frequencies of the individual combinations of the variants of two or more characters, so it is reciprocally possible to treat the combinations of variants of one and the same character in two or more individuals which are connected by known relations in a similar manner. This might be, for instance, when we wish to decide if a character is important in sexual selection; or again, if a character is hereditary or not. In the former case the combination of variants, effected by mating, of one and the same character in males and females, must show correlation; in the latter the same is true for parent and

offspring. Galton and Pearson have shown this in anthropological instances, but in zoology almost nothing has been as yet done.[1]

Statistical investigations may be applied to all sorts of characters; the immediate results acquaint us with the relative frequency of the variants, and show, in addition, whether their variation depends upon that of other characters or not. If we have to deal with numerical characters, we discover furthermore the particular law according to which their variants are distributed in the existing individuals of the form-unit, and the coefficient of correlation according to which the variants of several characters are individually combined. From the mathematical analysis of series of variation we discover constitutional factors, and the known external conditions of life differentiating the species into form-units and their higher groups, which are characterised in the first place by the mean values of their characters. Within the form-unit numerous other causes of variation, which are not known, produce by their combinations the individual differences of the characters in typical proportions of frequency. According to their physiological conditions the organs of different species react more or less markedly to the causes of variation of their characters, so that the physiological plasticity of the organs is indicated by the indices of variability of their characters.

The idea of investigating complexes of individuals statistically, in order to discover series of variation, is not new. In ichthyology especially, where nearly all systematic characters are dimensional or numerical, as early as 1857 A. Czernay [5] published observations on the variation of specific characters in freshwater fishes from the vicinity of Charkow. From the period 1870-1880 Heincke's [10] papers on the varieties of the herring may be named. All older publications, however, deal with such a small amount of material, that the data are without value for the mathematical analysis of series of variation.

In 1890, in the *Proceedings of the Royal Society of London*, there was published the first zoological paper where the results of statistical observations of numerical characters were mathematically analysed. W. F. R. Weldon [19] on the suggestion of F. Galton investigated four dimensions of *Crangon vulgaris* in numerous individuals from three different localities, and found that their variation follows the Gauss' law of error, which is a frequently occurring special case of Pearson's general probability-curve, and that each of the characters had a different mean value in the different localities. Two years later, using Galton's method, Weldon [21] showed the correlation between several characters of *Crangon*. Then he made a series of investigations on variation and correlation in *Carcinus maenas*, treating differ-

[1] Since the above was written, Warren [18] has published an interesting paper on heredity in *Daphnia*.

ences of age, sex, locality, in certain dimensions, and partly explaining them by selective processes [22, 23]. Further, he found a dimorphism of the females in the Naples race, which Giard [9] tried to explain by parasitic influences. In the meantime Thompson and Warren, inspired by Weldon, worked on variation and correlation in the dimensions of *Palaemon serratus* [15], *Carcinus maenas* [16], and *Portunus depurator* [17]. Warren first discovered the fact, afterwards several times confirmed, that the coefficients of correlation of homologous characters remain fairly constant not only within the form-units of the same, but also within those of allied but different species. Warren also was the first zoologist to follow Pearson's improved method in analysing series statistically. Thompson demonstrated distinctly determined changes of mean value and index of variability in the characters of the same form-unit in different years, which result Weldon has investigated further, and has recently [24] considered as a proof of the reality of natural selection.

While the leader of the English biological-statistical school is especially interested in the problem of natural selection, the North American school, led by C. B. Davenport, works especially on morphological problems. First, Davenport in association with Bullard [6] investigated in a very rich material (4000 individuals) the influence of sex on the constants of variation and correlation. On Davenport's suggestion Brewster [1] and Field [8], the first in mammals, the second in insects, investigated the relation between the variability of certain characters and their systematic importance. The result tended to show that the two correspond. But the material basis of these investigations appears to me too small to settle this question definitely.

Besides mathematical-statistical researches there have been published, since statistical methods came into vogue, some others of a non-analytical sort based upon large numbers of individuals. Among these I wish to call especial attention to Bumpus' papers on variation and mutation in two very different species introduced from Europe to North America, the sparrow (*Passer*) [2], and the periwinkle (*Littorina*) [3]. In each of these instances the great increase of variability in the American forms, compared with the European ones, is remarkable.

In Germany Heincke and I are still alone among zoologists in applying statistical methods to problems of variation. Heincke is chiefly interested in the existence of local races within the species, and one of his most important results is, I think, his method of determining the racial character of any given individual as well as its specific character [11]. Among botanists the number of fellow-workers increases every year. Besides foreign naturalists, H. de Vries and G. Verschaeffelt, who have published German papers, F. Ludwig has for several years been statistically investigating the law of Fibonacci in plants, while recently H. Voechting has published a splendid paper on abnormalities of flowers. There is a great advantage in botanical

objects, because they can easily be experimented on in regard to heredity and local variation.

If I have succeeded in showing the statistical method of investigating variation to be based on logical foundations, and to be capable, by its special nature, of yielding new and valuable results which cannot be acquired by any other method of investigation, it may lead, I hope, to the increased use of the method by zoologists. The mathematical training its application requires, does not exceed the standard of final high-school examinations. The exact and unambiguous results of the analytical method have a charm of their own, and it ought not to be forgotten that a precise terminology helps in every scientific work. The merely statistical research, whether directed towards aetiological or towards morphological problems, is only, I believe, the introduction to a more important kind of work, by which statistical-analytical and therefore critical results will be established with the help of quantitative experiments. To carry out this purpose we would need a separate institute, distinguished from the ordinary biological laboratory by larger accommodation for breeding experiments, and from the taxonomic museum by facilities for storing in an accessible fashion large quantities of homogeneous individuals which may readily be investigated at any time either for controlling or for completing former researches. To the eminently practical value of such an institution for agriculture, forestry, horticulture, as well as for fishery and cattle-breeding, I can now only refer; its chief aim of course would be scientific investigation, of which the results are always either directly or indirectly valuable to practical life. The first step, however, is that the statistical-analytical method be duly recognised as a new and important organon in the advancement of biology.

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18 HOFWEG, HAMBURG.

The Cereal Rust Problem—Does Eriksson's Mycoplasma exist in Nature?

By Geo. Massee, F.L.S.

The idea that the vegetative condition of parasitic fungi exists in the tissues of certain host-plants, and is transmitted from one generation to another, is not new. Berkeley (1), in discussing the subject, states as follows :—"The mycelium of the cereal fungi is known to exist from the earliest period in corn, and is perfected only under favourable conditions." Worthington G. Smith (2), in dealing with wheat rust, says: "They all prove that Puccinia is hereditary; that it exists in a finely attenuated state in seeds from diseased plants, and can be transmitted in a long interminable line from generation to generation." (3), "We, as well as many other observers, have shown that seeds apparently sound, will often, on germination, show disease in their seed-leaves; such plants are saturated with the germs of disease from their earliest period of growth." (4), Writing on the supposed hereditary nature of a disease affecting species of *Dianthus*, the same author states: "This case has a distinct bearing on the allied fungus of corn mildew, *Puccinia graminis*, which no one doubts is carried on from one generation to another in the seeds. This being the case, and nearly every grain of corn being probably saturated with the poisonous plasma of corn mildew, the statements regarding the production of mildew on corn from the contact of spores from a Barberry bush—the corn, it must be remembered, being almost invariably infested with hereditary disease —should be received with very great caution."

The above statements are not supported by experiments, but advanced as the only apparently possible explanation of the repeated occurrence of disease in those cases where external means of inoculation were not evident.

Quite recently Professor Jakob Eriksson (5), Director of the Experiment Station of the Royal Swedish Academy of Agriculture, has propounded a theory similar in substance to the ideas expressed by Berkeley and Smith, and bearing on the subject of rusts attacking cereals. The following quotations indicate the leading idea of the theory :—

"Plants of a variety of barley extremely liable to yellow-rust, which have been grown in sterilised soil in isolated glass houses, and have been protected against infection from outside, have sometimes become affected by yellow-rust."

"The yellow-rust appears in certain varieties of wheat and barley that are especially susceptible, uniformly four to five weeks after sowing."

"The results of these experiments prove beyond doubt that the disease must have come from an internal source, and have been inherited from the present plant."

"The fungus lives for a long time a latent symbiotic life as a mycoplasma within the cells of the embryo of the cereal plant, and only enters upon a visible stage in the form of a mycelium a short time before the pustules break out, and then only if the conditions are favourable."

Eriksson was gradually led to adopt the idea expressed above after prolonged study upon the succession of rust on cereals, a detailed account of which is to be found in another book by the same author (6); also in considering that the various forms of spores or reproductive bodies would not account for the amount of rust produced. The conclusions arrived at on this last point are summarised by the author as follows:—

"The germinating power of the uredo and aecidiospores is often small, or at best capricious."

"The germinating power of the winter-spores (teleutospores) depends upon certain external conditions, and is restricted to a short period of time" (5).

Now, if this theory proves to be true—that is, if it can be demonstrated that the protoplasm of a parasitic fungus can blend with the protoplasm of its host-plant, and remain passive in this condition from generation to generation until conditions are favourable for its manifestation in its own proper form as a parasite on the plant, in the protoplasm of which it has for a certain period of time remained inert—many unsolved problems in Vegetable Pathology would be easily explained, or, at all events, the discovery would afford a very feasible explanation of phenomena at present inexplicable. If this theory had been evolved some few years ago, it would undoubtedly have explained in a satisfactory manner the occurrence of the common "smut" of oats (*Ustilago avenae*, Jensen). An old idea was that the oat plant was inoculated by "smut" spores when in bloom, the fungus afterwards developing in the ovary. This idea being disproved, the mycoplasma theory would have been useful; now, however, that Brefeld's amply corroborated explanation of the life-history of "smut" has appeared, the necessity of mycoplasmic intervention has been superseded. The same will probably prove true in other instances. The weak point in the mycoplasma theory appears to be that it proves too much.

That Eriksson himself can only support his theory on negative evidence is shown by his own candid remark as follows (5):—"These results [those given above] prove beyond doubt that the disease must come from internal germs inherited from the parent plant. But in what form are these internal germs of disease living? Is it easy to follow and identify them with the microscope? Not at all. They can only be detected just before the breaking out of the young pustules."

A theory propounded by one who has for many years made a special study of the "rust" problem has naturally attracted much attention, and Eriksson's experiments have been repeated by observers in different countries, the result being in every instance opposed to the theory.

Bolley (7) holds that there is no ground for the mycoplasmic theory, his reasons for so doing being founded on similar experiments to those on which Eriksson founded his theory. Cereals were grown until quite mature, in structures specially arranged to prevent inoculation from rust spores, with the result that the plants remained perfectly free from disease, whereas every specimen of unprotected plants of the same kind growing close to the protected plants were badly rusted.

Mr. G. Nicholson, F.L.S., curator, Royal Gardens, Kew, kindly procured for me one pound of "Horsford Pearl" winter wheat. This variety was selected, because Eriksson says (5):—"We are warranted in suggesting that the predisposition of the Horsford wheat to yellow rust may be explained by assuming that between this variety of wheat and the yellow rust an extremely vital mycoplasma-symbiosis is to be found."

This was experimented with as follows:—

Half the quantity was used the first season. Two flower-pots, one containing ordinary Kew soil, the other old stable manure mixed with a very small quantity of soil similar to that used in the first pot, were prepared; no sterilisation was attempted in either case. An equal weight of wheat was soon in each pot. Each pot was placed on a large plate, a thick layer of cotton-wool was placed round the edge of each plate, and on this layer of cotton-wool a tall glass globe rested, each globe having an opening at the top plugged with cotton-wool. The glass globes were not removed for a period of twelve weeks, the necessary water being supplied by wetting the cotton-wool outside the bottom of the glass globe. At the expiration of twelve weeks the wheat in both pots had grown to the top of the glass globes, and in both pots was found to be perfectly free from "rust." At this stage these experiments terminated.

The remainder of the half-pound of wheat not sown in the plant pots was sown in a mixture of rotten manure and soil placed in a shady corner out of doors. At the expiration of nine weeks pustules of "rust" appeared on some of the leaves, and when the plants were about two feet in length nineteen per cent of the plants bore rust pustules on the leaves; but this "rust" on examination proved to be

black rust—*Puccinia graminis.* No trace of yellow rust—*Puccinia glumarum*—was present.

The following season the remaining half-pound of wheat was sown under conditions precisely similar to those described above. The plants protected by glass globes remained perfectly free from rust of any kind, whereas the seed sown on manure, and fully exposed to atmospheric conditions, showed at the expiration of thirteen weeks, twenty-eight per cent of rusted plants; the rust being *Puccinia graminis.* Not a trace of yellow rust—*Puccinia glumarum* — was present.

Remembering the clause in Eriksson's theory that the mycoplasma only assumes a visible form "if the conditions are favourable," I am ready to admit that both my out-door and other experiments with "Horsford Pearl" were not grown under the conditions necessary for the conversion of mycoplasma into mycelium, nevertheless my experiments are not unique in this respect.

McAlpine of Melbourne records having received from Eriksson ten varieties of wheat showing in a marked degree powers of resistance to yellow rust—*Puccinia glumarum.* When sown in Australia all the varieties were attacked by one or other of the native rusts—*Puccinia dispersa,* or *P. graminis.* No trace of yellow rust—*P. glumarum*—was observed (8).

These experiments corroborate at least what has previously been stated (9), that cereals especially susceptible to one form of rust in a particular country, may, if sown in another country, lose their susceptibility for the original kind of rust, and prove equally susceptible to another form.

As to whether this also proves that mycoplasma does not in reality exist, or that a change of locality destroys a mycoplasma that previously existed, I am not at present prepared to say.

Another set of experiments with wheat, commenced before the mycoplasma theory was published, were conducted for the purpose of endeavouring to ascertain whether mycelium passed into the seed in those cases where the mycelium of a parasitic fungus was undoubtedly present in the fruit.

This line of research was suggested by a remark made by Collenette (10), who in writing on the Tomato disease in Guernsey, says:—"My theory, then, is that the 'sleeping' disease is really primarily propagated by the seed, and the first thing to be done is to refuse to save or use the seed derived from the diseased plants." Collenette's theory was founded on the discovery of delicate hyphae in the tissue of tomato seed produced by a diseased tomato. I also had an opportunity of examining some seed obtained from a diseased tomato, kindly furnished by Mr. Collenette, and succeeded in detecting slender, hyaline hyphae about 2μ thick in the testa of the seed, but at the time was not able to demonstrate that these hyphae were genetically connected with

Fusarium lycopersici, Sacc., the fungus causing the tomato disease. This discovery was announced in a footnote to Collenette's paper quoted above. During further experiments with seed from diseased tomatoes sent by Collenette, I was able to corroborate the presence of slender hyphae in the testa of the seed, and furthermore obtained both the *Diplocladium* and *Fusarium* stages of the fungus by placing sections of the diseased seeds in a culture medium.

The above experiment leaves no doubt as to the fact of seed produced by a diseased tomato fruit being able to perpetuate the disease, due to the presence of latent mycelium—not mycoplasma—in the testa of the seed.

Experiments with Hollyhock seeds gave similar results. When the carpels are attacked by the Hollyhock rust—*Puccinia malvacearum*, Mont.—the testa of the seeds frequently contain mycelium, and such seed when sown, if it germinates at all, gives origin to a large percentage of diseased seedlings, the teleutospores of the fungus appearing on the hypocotyl and on the cotyledons in abundance. This experiment is of considerable importance, as the fungus belongs to the same genus as those producing rust on cereals.

Ustilago vaillantii, Tul., a fungus infesting the anthers, and sometimes also the ovary, of *Scilla bifolia*, and other allied plants, has been under constant observation for the past six years with the object of ascertaining its complete life-history, which is intended for publication in detail in the near future. The leading points in its history bearing on the question at issue are as follows:—Quite young seedlings may be infected by spores present in the soil. A perennial mycelium is formed in the short stem at the base of the bulb; from this hybernating mycelium hyphae pass into the flower-stalk each season; this mycelium finally reaches the anthers and the ovary. The mycelium is only present in the tissues at any given time for a length of about 2 mm.—in other words, as the mycelium creeps up the tissues of the flower-stalk it deliquesces and disappears behind, the growing tips of the mycelial strands only being at any one time evident, and when it has passed into the anthers there is not a vestige of mycelium to be found in the filaments of the anthers. All this takes place while the flower is in the bud condition, and the whole inflorescence is yet underground. When the fruit is attacked all the seeds are often completely destroyed, their position being occupied by a powdery black mass of fungus spores. In other instances only some of the seeds are destroyed, others present in the same fruit remaining apparently healthy; but on microscopic examination of such seeds slender mycelium can often be detected in the testa. Apparently healthy seeds obtained from a fruit having some of its seeds destroyed by the fungus, when sown in sterilised soil, and freed from adhering fungus spores by proper methods, always yield a large percentage of diseased seedlings, the mycelium soon being quite conspicuous in the delicate

stem before the bulb begins to form. This infection I consider to be due to the mycelium present in the testa of the seed. Unfortunately complete proof of this is not forthcoming as it was in the tomato disease described above, as up to the present moment no one has succeeded in causing any member of the Uredineae or Ustilagineae to produce fruit as a pure culture, or apart from the natural substratum.

In many instances the mycelium passes up the flower-stalk and enters the anthers and ovary without however producing spores, this final act being prevented by conditions at present unexplained. Mycelium can be frequently observed in the testa of seeds produced by such plants, and if the seeds are sown under conditions preventing external inoculation many diseased plants result.

Plants attacked in this manner can be easily recognised after a little experience, owing to the deep blue-green colour of the flower-stalk.

Returning to the experiment with wheat. Distinctly shrivelled grain caused by the presence of the rust called *Puccinia glumarum*, Eriks. and Henn., better known in this country as *Puccinia rubigo-vera*, DC., developed on the chaff, and sometimes also on the grain itself, was used. Forty grains were sown in each of two pots, one containing ordinary soil, the other rich stable manure with a small admixture of soil. Each pot was protected by a glass vessel with cotton-wool, as in the experiments described above. In the pot containing ordinary soil sixty per cent of the grain germinated, whereas in the richly manured pot only fifty-two per cent germinated. When the plants were three inches high indications of rust pustules were seen on a few leaves in each pot, and when the plants were five inches high twenty-six per cent of the plants were rusted in the pot containing ordinary soil, and forty-seven per cent in the richly manured pot. At this stage the experiments terminated, as the spores were in some instances mature, and the plants being crowded inoculation from spores would probably have taken place, and thus a greater percentage of rusted plants would have resulted than those due to what I consider as cases of rusting from the use of diseased seed. As a control experiment a similar number of plump and healthy grains obtained from plants having the foliage badly rusted, but the ears perfectly free from rust, were sown in ordinary soil and protected as described above. Ninety-six per cent germinated, and all the plants remained perfectly free from rust.

Another experiment was conducted as follows:—A jar was filled with sterilised water containing a small amount of extract of manure; a piece of coarse muslin was stretched over the top of the jar just in contact with the liquid. Twenty grains of wheat obtained from a plant not attacked by rust and presumably healthy were placed on the muslin, the jar being protected by a glass globe. Nine of the grains produced vigorous plants, the remainder being weakly were removed. When the plants were about two inches high a sterilised piece of

cotton-wool was loosely twisted round the base of each of three plants, extending up the plant about half an inch from the muslin on which the plants were growing. Three circles of stout white sterilised blotting-paper, each with a small hole in the centre and a slit from the hole to the margin, were prepared. One of these blotting-paper collars was placed round the stem of each of the wheat plantlets already enveloped at the base with cotton-wool, on which the blotting-paper rested, and was kept moist by the water conducted by the wool. Fresh uredo-spores of *Puccinia glumarum* were deposited in abundance, by means of a scalpel, on the damp blotting-paper at a distance of about one line from the stem of one of the plants; at a distance of about three lines from the stem in the second example, and in a circle about four lines from the stem in the third experiment.

Within a week of depositing the spores on the blotting-paper, the plant to which the spores were placed nearest drooped and fell over as in the disease known popularly as "damping off." Microscopic examination showed that death was due to a dense weft of mycelium emanating from the germinating uredo-spores that had surrounded the stem of the plant. I could not, however, demonstrate satisfactorily that any of the hyphae had penetrated the tissues of the wheat plant.

Within eighteen and twenty-two days respectively from the date of placing the spores on the blotting-paper, the two remaining plants showed uredo-pustules on the upper surface of the lowest leaf; in both instances the pustules appeared at a point about one inch above the blotting-paper. This, however, I do not hold to prove that the mycelium travelled upwards for that distance in the tissues of the leaf, but rather consider that the leaf increased an inch in length between the period of inoculation and the time the pustules first became visible externally. The remaining plants not inoculated remained free from disease.

The above experiment proves satisfactorily, I think, one point, namely, that it is not necessary that the uredo-spore should be in actual contact with the host-plant to insure inoculation, but that the germ-tube can live for some time as a saprophyte, when, if conditions are favourable, it can enter the tissues of a host-plant and assume parasitic functions. This feature may prove to be of great importance from the practical point of view in combating the disease. During the present season I hope to conduct further experiments for the purpose of ascertaining for how long a period the mycelium can grow as a saprophyte without losing its power of inoculating a host-plant, and also what distance it can traverse before effecting the same.

During the present spring an experiment was conducted on similar lines to the above, only teleutospores were used instead of uredo-spores. In this instance only one out of three infected plants produced uredo pustules, whereas an uninfected or check plant also showed

pustules, therefore no comment is necessary; only further experiments in the same direction will be made in the future.

Tradition acts as a powerful bias, even in scientific matters; immediately following De Bary's brilliant discovery of heteroecism, the condition of rust on the Barberry alternating with that on some graminaceous plant was considered indispensable for the continuation of the species; eventually it was discovered that the stage on Barberry could be dispensed with, and yet the rust appeared as rampant as ever; in fact in Australia, where rust is more abundant and injurious than in Europe, the aecidium condition is unknown; in India also, where ruts is very destructive, no aecidium condition is known to exist within hundreds of miles of the wheat-growing districts. At the present day it is generally accepted that the uredo-spores only retain their power of germination for a very limited period, and that the uredo-spores must be in contact with the host-plant to effect inoculation. The experiment just recorded modifies this idea to some extent. Teleuto-spores, again, are considered at present as being only able to infect the host that bears the aecidium stage; however, their production in such immense numbers in those countries where no aecidium stage is produced, or required, suggests that they may possibly play some part in the reproduction of the fungus hitherto undiscovered.

Numerous preparations of rust-shrivelled grains of wheat have been examined microscopically, and an abundance of mycelium detected in the outer layers of the grain, correctly speaking, in the pericarp; but not in a single instance have I been able to detect mycelium in the embryo; and in those cases where the grains were allowed to germinate and form a tiny plantlet up to half an inch in length, the mycelium never appeared to pass into this part. On the other hand, when sections of diseased wheat were placed in culture media, hyphae frequently radiated from the section on all sides for some distance.

May not similar hyphae radiate in the soil from diseased grain when sown under natural conditions, vegetate for some time in a saprophytic manner, and finally, if conditions are favourable, infect the young plantlet at, or just below, the ground level? Sufficient of obviously rust-shrivelled grain is frequently used as seed; and if, in addition, plants are infested with mycelium, which for some at present unknown reason does not produce spores, as I have shown to be the case with *Scilla bifolia*, and also recorded by Bolley (11) as frequently occurring in the case of wheat attacked by *Tilletia levis*, Kühn; and assuming that this mycelium also passes into the grain, then we should be able to account for a considerable quantity of the rust prevalent, without introducing a new factor—mycoplasma—into the theory.

Mycelium of the rust fungus has been observed in the grain of wheat by Eriksson, as shown by the following quotation (12), and if inoculation of the young plant is effected by means of mycelium originating from the grain, and growing for a longer or shorter period

in the soil previous to such inoculation, as explained above, then Eriksson's difficulty in accepting the presence of such mycelium as the cause of the disease, on account of its absence from the embryo, both before and immediately after germination, is removed.

"Ce fut en vain que je cherchai à constater, par le microscope, la présence de germes infectieux internes. Certainement je découvris dans les tissus périphériques des graines du froment ridées et déformées par la rouille, un mycélium très développé, et même parfois des espèces de nids des spores d'hiver (*teleutosporae*). Mais toutes les tentatives faites pour trouver un mycélium dans le germe lui-même, que ce fût dans le germe renfermé encore dans la graine, ou dans le germe sortant de la graine à la germination, restèrent infructueuses."

Many people have become so thoroughly accustomed to the annual loss of a certain amount of capital through "rust," "bunt," and "smut" of cereals, that it is looked upon as a matter of course; or, in other words, such loss is not realised at all; and it is only during seasons when these diseases are rampant that their presence is forced upon the cultivator, and even then only the amount of loss above the usual annual average is realised. The following figures, taken from official sources, illustrating the amount of loss sustained during an ordinary season, should be sufficient to explain why some governments have considered it incumbent upon them to aid in the endeavour to prevent such enormous losses.

"Oat smut (*Ustilago avenae*) alone destroys each year in the United States over $18,000,000 worth of grain. The other grains, especially wheat, rye, and barley, also suffer severely from smut diseases; the amount, however, has not been overestimated" (13).

In the same country we learn that "The aggregate loss from 'rusts' (*Puccinia* sp.) is estimated to be over $40,000,000 annually" (14).

The Prussian Statistics-Bureau states that the loss caused by "rust" alone on wheat, rye, and oats, in Prussia, during the season of 1891, amounted to a little over £20,000,000 (15.)

In Australia the loss in the wheat harvest of 1890-91, due to "rust," has been estimated at £2,500,000.

Finally we learn that in the United States, "Probably it would not be overstating the loss from plant diseases, as a whole in this country, to place it at $150,000,000 to $200,000,000 annually" (16).

The amount of annual loss in Great Britain arising from plant and animal pests is not officially estimated, but it may safely be assumed that, if half the amount of loss could be prevented, farming and horticulture would prove to be remunerative occupations.

An equally formidable array of figures could be quoted from official publications showing the actual gain derived by following the directions issued from experiment stations.

The question that naturally suggests itself at this point is the

following:—How is it to be explained that in countries where experiment stations are most numerous, and information on every question to be obtained without delay, that the annual loss arising from those identical causes which it is the avowed object of such institutions to assist in preventing, is still so great? The answer is, officials of experiment stations can give valuable information, but—except in the case of certain diseases, and then only in limited areas—cannot enforce the carrying out of the necessary measures for their prevention.

The first and greatest difficulty that those who essay to teach cultivators of the soil how to avoid loss from the attacks of plant and animal parasites, have to contend with is, that of replacing prejudice by intelligence; and this is perhaps more especially true of old countries, where you are confronted by statements showing how somebody's great-grandfather made a fortune out of farming without having recourse to any of the methods now advocated.

Tact is undoubtedly necessary, but actual demonstration is the sheet-anchor of success; consequently, as has been realised in many countries, experiment stations are indispensable, where actual results can be seen. Literature, as a supplementary factor, is of undoubted value, but too much reliance should not be placed on this feature during the initial stage of conversion.

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THE HERBARIUM,
ROYAL GARDENS, KEW.

Problem of Honeycomb.

By Charles Dawson, F.G.S., and S. A. Woodhead, B.Sc., F.C.S.

The hexagonal arrangement of the cells of honeycomb has been generally ascribed to a structural instinct on the part of the bees; the object of this paper is now to show that the form of the bee-cell is chiefly influenced by a "crystalline" hexagonal formation due to the cooling of the wax.

While experimenting with waxes and resins one of us (Mr. Dawson) noticed that on cooling the mixture had a tendency to arrange itself in hexagonal forms, from which he surmised that the outline of bee-cells might be primarily due to the natural structure produced in cooling wax. At the instance of Mr. Woodhead, who also recognised the analytical importance of such a discovery, it was agreed to work out the details together in Mr. Woodhead's laboratory at the Agricultural College, Uckfield.

It was first of all determined that although the addition to beeswax of resinous substances gave a more pronounced and bolder outline to the hexagons, no such addition to beeswax was necessary for their production.

If a thin slab of beeswax be melted in a shallow tray (measuring, say, 10 × 8 inches), which is evenly heated throughout, and is then placed to cool gradually in a warm atmosphere without draught, hexagonal crystalline forms, of the ordinary size of a worker-cell of the hive-bee, will be seen gradually forming at the bottom of the dish. And a similar line of hexagons will be seen to form on the surface of the wax round the sides of the dish where the wax first cools. The sides of the hexagons are to be seen forming and branching out in advance of the cooling wax, and when a portion of the wax in the centre of the dish alone remains melted, the remaining hexagons form very rapidly and almost appear to flash out upon the surface.

The tray should be exactly level and the wax about 1·5 mm. thick and of uniform depth, and the atmosphere of even temperature (say a few degrees below the melting-point of the wax), otherwise the hexagons will be irregular in size and shape.

It is immaterial how thin the plate of wax is, as the hexagons are

formed in any case, but their size is undoubtedly regulated by the thickness of the plate of wax, the rule being the thinner the plate the more minute the individual hexagon. The same result may be obtained on a much smaller scale so as to produce only one or two hexagonal forms, but the operator will then find that the difficulty lies in the rapid cooling at the sides of so small a mass of wax.

The explanation of the formation of these hexagonal bodies is as follows:—

On cooling, the wax at first forms into nuclei of nearly equal size. On the shrinking of the wax by further cooling, these nuclei or spheroids are pressed together, forming planes at their points of contact. Should the wax be rapidly chilled before these spheroidal bodies are formed to their full extent, they are then prevented from coming into contact one with another by the intervening nebulous masses of "uncentralised" particles of wax. It would appear by microscopic examination that these particles are also smaller nuclei which become absorbed in the larger. They also, like the larger, assume hexagonal form. In this state the nuclei appear when cold as solid circular bodies.

The hexagons appear very distinctly above and below the surface while the wax is cooling. When it is actually solid, their forms are often very indistinctly seen, or may be altogether invisible, but they are none the less present. The bases of these hexagons, which lie mid-way between those visible at the top and those at the bottom, are pointed and are arranged so that the point of the base of the upper hexagon coincides with the points of contact of the lower hexagons as in the honeycomb. These bases can be observed by making a very thin microscopic section, but several hundred sections had to be examined before they were made out with certainty.

When a small amount of resin and turpentine is added to beeswax and melted, and the mixture is allowed to get cold, the outlines of the planes of contact on the hexagons are more distinct and are to be seen raised upon the surface. Under these circumstances they may be easily rubbed with black lead, which still further increases their visibility.

Our chief experiment was next to put our theory to a practical test, and observe in what manner the bees would deal with a cast sheet of pure beeswax, which, when viewed by a side light, distinctly showed traces of these natural hexagons over its surface.

Before introducing it to the bees, we had traced upon it with vermilion a group of the hexagons which appeared near the centre of the plate. (Another group we black-leaded.) This was then photographed, after which the wax plate was placed in an observatory-hive on a bar-frame. The bees soon started upon it, proceeding to excavate round hollows in the centres of the hexagons, at the edges of the plate, pushing out on all sides the *débris* around the edge of each

excavation. When they reached the planes of contact of the hexagons, either on feeling the minutely raised edges on the surface, or more probably on feeling the increased density of the wax, the bees determined the limits of their excavation; and it was then discovered by us that the bases of these hexagons were three-sided in the usual form of a bee-cell. There are two reasons for the density of the wax, namely, the outer edges of the nebulæ are composed of smaller particles and are therefore more compact, also the pressure brought to bear on the planes of contact renders the sides of the bodies still more compact. Meanwhile, a similar process was going on in the cells which lay as nearly as possible in the same irregular wavy line, but the work on one side of the sheet was sometimes considerably more advanced than on the other, the excavation being brought three or four more rows of cells nearer the centre on one side than on the other.

Portions of the *débris* taken from the centre of the hexagon were now kneaded up by the bees into a kind of froth, and placed above the lines of pressure or margins of the hexagons, the residue of the *débris* being put aside for future use.

The portions placed on the margin of the hexagons speedily adhered and solidified, another layer was then added by the bees, and this process was repeated, thus forming a series of strata (which may be noticed under a magnifying glass on the sides of the complete cells); the bees planing and polishing the inner surfaces of the cell upwards from the base, taking as guides the planes and angles of the hexagons.

In the places where we had traced the outlines of the hexagons in vermilion, the bases of the cells were to be distinctly seen formed upon the vermilion outlines.[1] Similar experiments have been repeatedly tried with the same results.

In places where the wax plate had been of uneven depth, or had cooled too rapidly, the comb presented an irregular appearance following in form the irregular "crystalline" bases beneath, the result being very distinctive and striking to the practised eye of an apiarist.

When in a natural state, the newly secreted wax is formed into a small pendent plate, it is probable that the bees crowding around produce the required amount of heat to soften or to keep soft the newly deposited wax, and allow it to cool very gradually when a few "crystalline" bodies form within the plate, and these must be soon afterwards hollowed out and built upon. The same process takes place repeatedly against the sides of newly formed hexagons until the comb is large enough to suit the requirements of the bee; the sizes of the cells being partly influenced and regulated as above stated by the rapidity or otherwise of the process of cooling of the wax, and so

[1] A plate of wax formed by compression, and in which no hexagons had formed, was inserted in the hive—this the bees gnawed to pieces and (?) utilised elsewhere.

indirectly, as previously mentioned, by the thickness of the cooling mass. The size of the hexagons may be varied experimentally from those of nearly an inch across to others of microscopic dimensions.

At the time of writing this paper, we have not yet succeeded in casting a large sheet of wax containing groups or rows of hexagons so perfectly regular as those which are to be seen in a natural comb, or in a comb built upon the ordinary manufactured comb-foundation. We do not pretend, even after many experiments, to be able to cast a foundation of hexagons with the same comparative exactitude as those made by a bee. Although we have little doubt that we may soon be able to do so, we cannot expect, in a few limited experiments, to compete with the bee, whose seeming aptitude is probably the outcome of ages of natural selection and adaptation. Yet the bees still prefer to adopt our less regular groups or rows of hexagons as bases to work upon, rather than pull our wax plate to pieces, so as to recast the wax with greater regularity.

A further outcome of our discoveries is that paraffin wax and adulterated beeswax do not assume the same "crystalline" form as pure beeswax.

We have succeeded in producing a variety of characteristic forms of these "crystalline" bodies by the treatment of certain waxes with other fats, oils, or waxes. The analytical value of these experiments we may hope to prove to be very great, both directly and indirectly, and to open up an immense field of crystallography in its relation to oils, fats, and waxes.

It has also naturally occurred to our minds that the formation of certain intricate structures by other insects may be also more or less directly due to crystalline and pseudo-crystalline formations.

UCKFIELD, SUSSEX.

The Supposed Existing Ground-Sloth of Patagonia.

By A. Smith Woodward.

Much interest was aroused a year ago by Dr. Ameghino's announcement in *Natural Science* of the discovery of a piece of skin of a ground-sloth in Patagonia.[1] He supposed the specimen to belong to a small surviving representative of the gigantic extinct ground-sloths which were so abundant in the Pleistocene period in South America, and were known to have existed at least until the appearance of man in that country. Dr. Ameghino thought that this piece of skin might have belonged to a mysterious animal which had been described to him by the traveller Ramon Lista, so he named the new creature *Neomylodon listai*. With admirable conciseness he pointed out the main features of the skin—how it was completely covered with long dense hair, while being at the same time armoured by a close pavement of small nodules of bone embedded in the lower layer. He also quite correctly recognised that the bony armour was most closely paralleled by that dug up with the skeleton of the great extinct *Mylodon* in the Pampa formation in various parts of the Argentine Republic.

More precise details of this discovery were subsequently published by Dr. Moreno, Director of the La Plata Museum, and by Dr. Otto Nordenskjöld of Upsala; while a technical description of the skin itself was prepared by Dr. Einar Lönnberg and myself.[2] These additional communications showed that the specimen in question was dug up in the dust of the floor of a large cavern near Last Hope Inlet. They also seemed to prove that *Neomylodon listai* must have been at least as large as the well-known *Mylodon*—that is, not less in bulk than a rhinoceros. Notwithstanding the fresh aspect of the piece of skin, it thus appeared extremely improbable that the animal was still living, and had escaped the notice both of the natives and of explorers. Dr. Moreno, indeed, maintained that it was quite extinct, and dated back to a time when a former race of men, unknown even to the present Tehuelches, inhabited the southern extremity of the South American continent.

[1] F. Ameghino, "An Existing Ground-Sloth in Patagonia," *Natural Science*, vol. xiii. p. 324 (Nov. 1898).

[2] See *Natural Science*, vol. xiv. p. 265 (April 1899).

The Director of the La Plata Museum, with the characteristic energy which has established the fame of that great seat of learning, determined that no time must be lost in solving the problem of *Neomylodon*, so far as careful explorations could accomplish it. Dr. Rudolph Hauthal was accordingly deputed last April to undertake further diggings in the "Cueva Eberhardt," as the now celebrated cavern is named, and the results, just published, prove to be of the deepest interest.[1] These further discoveries include nearly all the important parts of the skeleton of the animal, evidently broken by man and clearly associated with relics of man himself.

It now appears that the remains of the so-called *Neomylodon* are not found at the exposed entrance of the cavern, which is of very large proportions (30 metres high), but occur only in an inner chamber which has every appearance of having been artificially constructed by cross-barriers. At a short distance from the entrance there is a rude wall of tumbled blocks extending the whole way across, except a narrow gangway left at one side. On passing through this the great chamber just mentioned is reached, and another wall-like barrier 50 metres further inwards extends completely across the cave from side to side, preventing any ingress except by scrambling. In the middle of the chamber there is an artificial mound. The floor proved to be covered with a layer of dust and stones, varying from 30 centimetres to a metre in thickness. In it at one spot were found numerous shells of mussels mingled with the broken bones of guanaco and deer—evidently the remains of the food of man. Beneath the surface layer near the inner barrier was discovered a great mass of excrement of a herbivorous animal, in some places more than a metre in depth. Most of the material was in the form of impalpable dust, which almost choked the workmen; but a few large lumps were in a good state of preservation, and rivalled the droppings of the elephant in size. Part of the heap showed clear indications of having been burned. Nearer the middle of the chamber was dug up a considerable accumulation of dry cut hay in a good state of preservation. In the lower layer—in the excrement, the hay, and the surrounding rubbish—were found numerous broken bones of the so-called *Neomylodon*, belonging to several individuals, both old and young, with another well-preserved piece of skin. There was also evidence of an extinct horse, and a large unknown carnivorous animal; while a human skeleton had previously (in 1895) been taken out of a niche in the wall of the chamber.

Summarising the results of his work, Dr. Hauthal specially emphasises the following facts:—

"1. That the deposit of excrement was confined to the space

[1] R. Hauthal, S. Roth, and R. Lehmann-Nitsche, "El Mamífero Misterioso de la Patagonia, 'Grypotherium domesticum,'" *Revista del Museo de La Plata*, vol. ix. p. 409, with five plates (Aug. 1899).

between the inner barrier and a mound—a space which could easily be shut off.

"2. That at the foot of the mound inside, but a little behind the excrement, there was found a considerable quantity of cut hay beneath the same layer of earth and stones which covered the excrement; while this hay could only have been placed in this situation by man.

"3. That the aspect of the layer of excrement indicates the existence of a stable, exactly as if it had been an old corral."

He thus concludes "that the men who lived there ages ago were accustomed to stable their domestic animals in this part of the cavern, reserving the rest for their own dwelling-place."

This extraordinary idea leads us to turn with expectant interest to the fragmentary remains of the so-called *Neomylodon;* for if the beast was a gigantic ground-sloth, it is inconceivable that so unwieldy a monster can have been of any use to man as a domestic animal or of any value to him except as food. The descriptions and figures published by Dr. Santiago Roth leave no doubt whatever that the quadruped in question was a gigantic ground-sloth; and the so-called *Neomylodon* is clearly proved to be identical with a *Mylodon*-like animal, already well known by the skull from the Pampa Formation of Argentina, described under the names of *Glossotherium* (Owen, 1840) and *Grypotherium* (Reinhardt, 1879). It is, in fact, a *Mylodon* with a very long head and laterally-placed nostrils. The species from Cueva Eberhardt is probably distinct from the *Glossotherium* (or *Grypotherium*) *darwini*, and will thus be known for the future as *Glossotherium listai.* After a ridiculous line of argument, which one would hardly expect to find in a scientific treatise, Dr. Roth proposes to change the specific name; but this point needs no discussion.

By the kindness of Dr. Moreno, the actual skull discovered by Dr. Hauthal and some pieces of the excrement were exhibited to the British Association at Dover; and the specimens will be further discussed at a forthcoming meeting of the Zoological Society of London. The animal must have been killed by man, for the cranium is battered on the top in three places. The blows themselves would probably merely stun the creature, for the air-chambers above the brain-case are too extensive to permit injury of the brain from above; but the men clearly had knives or sharp instruments of some kind, for there are distinct clean cuts on the remains. Pieces of periosteum, cartilage, ligaments and dried muscle still adhere to the bones. The specimens have a peculiar odour, and three of them exhibit no indications whatever of having been buried. Presumably these were dug out of the hay. They are, indeed, so fresh, that if the discoverers had reported that the animal had been killed shortly before the bones were packed up, the evidence of the specimens themselves would not have sufficed to contradict the story.

The excrement of the animal is of great interest, and was examined

by Mr. Spencer Moore in view of the British Association meeting. He reports that it "consists in large part apparently of grasses, as the haulms, leaf-sheaths, fragments of leaves, etc., of these plants are frequent in it. A spikelet, almost entire, of what seems to be a species of *Poa*, and the flowering glume of another grass, probably Avenaceous, have also been found. Besides these there is at least one dicotyledonous plant, almost certainly a herb, with a slender greatly sclerotised stem; though, as no attached leaves have so far been observed, its affinity is altogether doubtful." Mr. Moore also observes that there are numerous siliceous particles in the excrement, and several pieces of the underground parts of the plants, as if they had been pulled out of the ground. At the same time, he finds a few pieces which have been sharply cut in a way which the blunt teeth of *Glossotherium* (*Neomylodon*) could scarcely act. Since Owen's well-known and beautiful memoirs on *Megatherium* and *Mylodon*, it has always been supposed that the gigantic extinct ground-sloths fed on twigs and the leaves of trees. If his conclusions are well-founded, as seems almost beyond dispute, *Glossotherium* must either have been an exception to the rule owing to local circumstances, or it must have been doomed to an artificial mode of life by man who fed it. The authors of the memoir published by the La Plata Museum are all in favour of the latter view; and Dr. Lehmann-Nitsche even suggests that the famous cracked and repaired skull of *Mylodon* in the Royal College of Surgeons, immortalised by Owen, was not accidentally damaged by a falling tree, but bears the mark of an encounter with man in which the animal escaped. He mentions five similarly fractured skulls in the La Plata Museum.

Personally, we find it as difficult to believe that *Glossotherium* was a domesticated animal among the ancient Patagonians, as that it still lives in the wilds of the southern land where its remains are found. Dr. Hauthal's splendid discoveries only have the effect of making us eager for more. Mr. Graham Kerr's interesting speech at the British Association, expressing the opinion of one who has considerable experience of the South American Indian tribes, leaves little hope that huntsmen will ever find the beast. The Indians, in his opinion, are too keen field-naturalists to have escaped noticing the animal if it lives in their country. They know every track and trail. The impalpable character of the dust in the cave alone suggests intense dryness, and strongly confirms Dr. Moreno's idea that all the remains in Cueva Eberhardt are of great antiquity, notwithstanding their fresh aspect. More cave exploration in southern Patagonia is therefore urgently to be desired.

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FRESH FACTS.

A Strange Tail. Gustav Tornier. "Ein Eidechsenschwanz mit Saugscheibe," *Biol. Centralbl.* xix. 1899, pp. 549-552, 3 figs. The end of the tail of the lizard *Lygodactylus picturatus* is unique. It bears twenty attaching plaits in two rows, which form an effective sucker on the vacuum principle. The fingers and toes bear similar plaits, but each has only half as many plaits as the tail. The strange tail is an adaptation for clambering on the smooth surfaces of bananas and candelabra Euphorbias.

Fauna of Frog Spawn. Carl Thon. "Einige Beobachtungen über die Fauna, welche sich im Froschlaich aufhält," *Verh. Zool. Bot. Ges. Wien*, xlix. 1899, pp. 391-393. In ponds from two different localities in Bohemia, Thon found that the spawn of *Rana fusca* and *R. esculenta* had associated with it an almost identical set of small animals. A few days after hatching, small Dyticidae, *e.g.*, *Hydroporus*, made their appearance, but were not seen to injure the eggs; then water-mites, *e.g.*, *Eylais setosa*; then Entomostraca, *e.g.*, species of *Cyclops*, *Chydorus*, and *Cypris*, some of which helped to loosen the jelly. After hatching, many insect larvae appeared, *e.g.*, of *Cloëon dipterum*, *Ceratopogon*, *Chironomus*, *Perla*, *Limnophilus*, some of which devoured the young tadpoles greedily. Below the spawn lay *Asellus aquaticus* back downwards; nymphs of *Curvipes*, etc., were also abundant. Among the tadpoles, but hardly distinguishable because of their dark colour, were individuals of *Polycelis nigra*. After the empty spheres sank to the bottom, some encysted Vorticellids, many monads and diatoms, some statoblasts and ephippia were found amongst the jelly, but no infusorians or rotifers. Some of the associates loosen the jelly, others effect its further dissolution; others, again, make war with the tadpoles, but the protective value of the jelly is corroborated.

Branchial Respiration in Millipedes. M. Causard. "Sur la respiration branchiale chez les Diplopodes," *Comptes Rendus Acad. Sci. Paris*, cxxix. 1899, pp. 237-239. The observer found *Brachydesmus superus* in a brook under submerged stones, and was interested to notice that it evaginated two transparent ampullae from the rectum. He put *Polydesmus gallicus* in water, and observed the same phenomenon, and he succeeded again with a species of *Iulus*, so that the occurrence is probably not infrequent. The ampullae are formed from a protrusible rectal pouch, hitherto unobserved, and as they show tracheae and blood-currents, Causard does not hesitate to speak of a branchial respiration.

A Reducing Ferment in the Animal Organism. E. Abelous and E. Gérard. "Sur la présence, dans l'organisme animal, d' un ferment soluble réducteur. Pouvoir réducteur des extraits d'organes," *Comptes Rendus Acad. Sci. Paris*, cxxix. 1899, pp. 164-166. In extract of horse's kidney a soluble ferment was found which reduced potassium and ammonium nitrates, decolorised methylene blue, and seemed to form butyric aldehyde from butyric acid.

Breeding Habits of a Tree-Frog. J. S. Budgett. "Notes on the Batrachians of the Paraguayan Chaco, with observations upon their breeding habits and development, especially with regard to *Phyllomedusa hypochondrialis*, Cope; also a description of a new genus," *Quart. Journ. Micr. Sci.* xlii. 1899, pp. 305-333, 5 pls. The author observed a female of *Phyllomedusa hypochondrialis*, with a male upon her back, wandering about in search of a leaf whereon to lay her eggs. "At last the female, climbing up the stem of a plant

near the water's edge, reached out and caught hold of the tip of an overhanging leaf, and climbed into it. With their hind legs both male and female held the edges of the leaf, near the tip, together, while the female poured her eggs into the funnel, the male fertilising them as they passed. The jelly in which the eggs were laid was of sufficient firmness to hold the edges of the leaf together. Then moving up a little further more eggs were laid in the same manner, the edges of the leaf being sealed together by the hind legs, and so on up the leaf until it was full. As a rule two briar leaves were filled in this way, each containing about 100 eggs." Even more interesting, however, is the subsequent development.

HOW COPEPODS SWIM! E. W. MACBRIDE. "The movements of Copepoda," *Quart. Journ. Micr. Sci.* xlii. 1899, pp. 505-507. In the freshwater *Cyclops* the first antennae assist in the slow movements, and the belief is general that copepods propel themselves by their first pair of appendages. Prof. Macbride observed at Plymouth that the slow gliding movements of marine copepods are effected principally by the second antennae, the gnathites likewise assisting, notably the second maxillae. The quick movements, on the other hand, are effected entirely by the simultaneous action of the thoracic feet.

CLAMPS IN ANIMALS. OTTO THILO. "Sperrvorrichtungen im Tierreiche," *Biol. Centralbl.* xix. 1899, pp. 504-517, 13 figs. Dr. Thilo points out that one must serve some apprenticeship in engineering before one understands the animal body, and his ingenious essay bears this out. He leads us from the valves of the heart to the device which keeps the globe-fish's self-inflation from collapsing, but he is at his best in expounding clamps for rigid structures. From the clamp of the spine of *Monacanthus* (a fish from the Red Sea coral-reefs), we pass to more complex cases in *Triacanthus* and the stickleback, and the leverage-system which works the snake's fang is not forgotten. It is an essay for a dull afternoon, so ingenious is it; but it is with some misgivings that we are forced to conclude that in addition to mathematics and meteorology, statistics and spectroscopy, psychology and philosophy, and much more, the complete naturalist must also learn engineering.

VARIATIONS IN JELLYFISH. E. BALLOWITZ. "Ueber Hypomerie und Hypermerie bei *Aurelia aurita*, Lam.," *Arch. Entwickelungsmechanik*, viii. 1899, pp. 239-253, 1 pl. This common jellyfish seems to be an animal well deserving the attention of those who follow the modern statistical method of the study of variations. It is normally a tetra-partite creature, but sex-partite, pent-partite, and, more rarely, tri-partite forms may be found thrown up on the beach. Sometimes the variation is very consistent throughout; thus a tri-partite individual had a three-cornered mouth, three genital pockets, six marginal bodies, etc.; but, often, there is less uniformity and transitional forms occur. Some of the variations may be traceable to the Ephyra-stage, but most, according to Ballowitz, must have an earlier origin. Here is evidently a case for experiment to assist observation.

DIGESTION IN FISHES. EMILE YUNG. "Recherches sur la digestion des poissons (Histologie et physiologie de l'intestin"), *Arch. zool. expér.* vii. 1899, pp. 121-201, 1 pl. Prof. Yung has made many histological observations and physiological experiments in regard to digestion in fishes, and has removed some of the prevalent vagueness. The formation of pepsin seems rigidly confined to the stomach-sac and to a particular region of it.

THE PROBLEM OF EQUILIBRATION. TH. BEER. "Vergleichend-physiologische Studien zur Statocystenfunction. ii. Versuche an Crustaceen (*Penaeus membranaceus*)," *Pflüger's Arch. f. Physiol.* lxxiv. 1899, pp. 364-382. When the statocysts of *Penaeus* are extirpated, the animal can no longer keep its balance in swimming; it falls to one side or to the bottom.

SOME NEW BOOKS.

SCHARFF'S EUROPEAN FAUNA.

The History of the European Fauna. By R. F. SCHARFF. Contemporary Science Series, 1899. Pp. vii. + 364, Illustrated. London: Walter Scott, Ltd. Price 6s.

For many years Dr. Scharff, of the Dublin Museum, has been turning his attention to the important question of the origin and relations of the existing fauna of Europe. And the present volume, which includes the substance of a paper previously published, embodies the results of his investigations so far as they have been hitherto carried. Whatever may be the precise value of such results and conclusions, it may be unhesitatingly conceded that it is a great convenience to workers to have them in the form in which they are now presented. One great and praiseworthy characteristic of Dr. Scharff's work is to be found in the thorough manner in which he has looked up and quoted previous observers on the subject; and, if for no other reason, the little volume before us will always have a very considerable value on account of the views and opinions of a host of specialists which are brought together and contrasted and correlated. Moreover, the author has drawn his conclusions from almost all groups of animals, although admitting that the evidence derived from certain of these groups is entitled to much more weight than that afforded by others. In regard, then, to the perseverence and energy which he has brought to bear on a very difficult task, Dr. Scharff is clearly entitled to our best congratulations.

But whether he has succeeded in establishing the views he holds in such a manner as will lead to their general acceptance, is quite another matter.

As the author correctly points out, the fauna of Europe, as a whole, is a complex, including a mingling of essentially Arctic types with those of a Lusitanian or Mediterranean origin, as well as those characteristics of the heart of the area itself. Moreover, Dr. Scharff likewise accepts the view that a Siberian, or north-east Asiatic, element has been introduced into the fauna. With all these we are prepared to agree; but we venture to think that the author is much too fond of drawing wide-reaching conclusions from a very small amount of fact. Especially is this the case with regard to the "migrations" of which he is so constantly speaking. As an instance of what we mean, we may refer to the common hare and the wild boar, both of which are regarded as "Oriental immigrants" into Europe. Now, without venturing to deny that the author may be right in this contention, we do not hesitate to say that he has not adduced any evidence which is entitled to a moment's consideration in favour of such a view.

But in other instances it is not want of evidence that we have to deplore, but an actual misapprehension of the facts. The most glaring case of this is afforded by the inductions drawn from the reindeer of Europe. Here it is stated that two types of reindeer occur fossil in Europe, one of which, together

with the existing Scandinavian animal, is regarded as practically identical with the barren-ground reindeer of Arctic America, while the other is considered inseparable from the woodland reindeer of North America. The former of these, it is said, is found only in the extreme west of Europe, while the latter occurs in Central and Eastern Europe and Asia. And on this evidence it is argued that the barren-ground reindeer entered Europe by a land connection *viâ* Greenland and Iceland; while the woodland form made its way *viâ* Bering Strait.

At the conclusion of a very long argument he notices (p. 157) that a recent writer has denied the identity of the Scandinavian and the barren-ground reindeer, and then he proceeds to remark that "the whole subject is by no means as well known as could be wished, and a very careful comparative study of recent and fossil remains of the reindeer from various parts of the Old and New World, is much needed to put our views on a firmer basis."

This paragraph, coming after the conclusions definitely drawn as to the Greenland and Bering Sea routes, is equivalent to saying that so long as Dr. Scharff's views of the relations of the Old and New World reindeer are followed, everything is settled, but if anyone else ventures to take a different view, then the whole matter requires investigation (with the object, we presume, of re-establishing the Scharffian interpretation).

As a matter of fact, the Scandinavian reindeer, as all American naturalists are agreed, is a perfectly distinct animal from the barren-ground form; the only difference of opinion being as to whether they should be regarded as species or races. If Dr. Scharff is right in considering that there were two types of reindeer in Europe, their distribution may be perfectly well explained by assuming that the western or Scandinavian form wandered from Scandinavia by a land connection between that country and Scotland, and so on to Ireland, at a time when England was detached from Scotland and joined to the Continent. On the other hand, the second form might have spread over the whole of central and eastern Europe, and thus through Asia to America. There are no grounds, however, for deciding whether the Old or the New World is the original home of reindeer.

The author further assumes that the Irish stoat accompanied the so-called barren-ground reindeer into Europe by the Greenland route; while the English stoat arrived from Asia. The former can, however, scarcely be regarded as anything more than a race of the common stoat which has been isolated for a longer period than has its English representative. Consequently, although, as indicated by the plant evidence, there may have been means of communication at an earlier date, we fail to see any evidence for a land connection between North America and Europe by way of Greenland at the time when reindeer flourished in our own country,—that is to say, during, or just previous to the human period. Without any intention of rudeness, we may indeed suggest that writers should use common sense in matters of this sort; for the conclusions referred to are, in our opinion, sadly wanting in that very useful commodity.

Many other cases might be criticised, but the above is sufficient to show that all the author's conclusions are not to be taken as gospel.

In reading the book we have been much irritated by the author's fondness for repetition. For instance, Dr. Bonney is quoted no less than three times in support of the view that the boulder-clay may be a marine deposit; on pages 83 and 229 the very same passage is quoted at length twice over, while on page 180 it is paraphrased. As another example, we are thrice told (pp. 79, 185, 239) that Arctic and Alpine plants have to be protected in winter on the lowlands of Britain and the Continent. Moreover, in several places, there is, in our judgment, a want of clearness of expression in more than one passage. And there are not wanting instances of carelessness, as for instance, *barbarus* in lieu of *barbatus* on page 46. Then, again, we have always been under the impression that the genus *Agama* is the type of a family, and that it has no claim to

be regarded as a member of the Iguanidae (p. 193). Neither are we aware what animal is meant by the "Siberian Red Deer" (p. 249); but then (p. 248) the author does not appear to be aware of the essential distinction between a Red Deer and a Wapiti!

Should a second edition of what is in many respects a very interesting work be called for, we venture to hope that the author will modify some of his conclusions in regard to migration and former land connections, which appear to us to set probability at defiance.

AGRICULTURAL PROGRESS IN AMERICA

Year-Book of the United States Department of Agriculture, 1898. 8vo, pp. 768. Washington: Government Printing Office, 1899.

"The American Agricultural Year-Book" for the past year fully maintains the high reputation which the Department has justly earned by previous volumes of this publication. It is divided into three parts—(1) The Report of the Secretary of Agriculture to the President; (2) Miscellaneous Papers by chiefs of bureaus, divisions, and officers of the Department, or their assistants; and (3) An Appendix consisting of a summary of useful information. Five hundred thousand copies are annually printed and distributed, and so great is the demand that the Secretary recommends the increase of the current year's issue by 20,000 copies. Secretary James Wilson, or, as he would be designated in this country, Minister for Agriculture, is a native of Ayrshire, and springs from the same stock as the late Dr. M'Cosh of Princeton University. He left Scotland at the age of sixteen, and has, through his sterling worth and devotion to the best interests of agriculture, raised himself to the high position which he fills with much credit alike to himself and to the State. The Department is divided into over twenty distinct sections, each being worked by a staff of well-trained specialists. The Secretary's report refers to the leading results of the year's investigations, but we can mention only a few of the more important of these. We are told that the Department is searching the world for seeds and plants to diversify the crops of the country, and to add new varieties to meet sectional requirements. Four scientific explorers are abroad getting seeds and plants from Russia, around the Mediterranean, China, and South America. Of grasses, no less than 500 varieties are grown for educational purposes in the gardens of the Department. The Bureau of Animal Industry has discovered a substance which by means of one dipping will destroy all ticks infesting an animal, so that at last a remedy has been found to prevent the spread of Texas fever among cattle. Inoculation with antitoxin serum for the prevention of hog cholera has for two successive years saved 80 per cent of the animals treated, while as many as 80 per cent of the check herds not treated died. Important additions have been made to the Department library, which now contains nearly 65,000 volumes, and forms one of the largest collections of books on agricultural topics in the world. "Nature-teaching" in the common schools is receiving the special attention of the Department, as well as the great prerequisite, the education of the teacher. This is the natural development following the experience of what it is possible to do in agricultural colleges to meet the requirements of the country. In this connection America is immeasurably ahead of this country, where educational authorities have practically discarded the country schoolmaster as a teacher of agriculture, and are wastefully spending public money in duplicating agricultural colleges which are already far in excess of the requirements of the country, and are in the aggregate more than half empty.

The Weather Bureau is a most important and well-equipped section of the Department. So numerous are the Observation stations in all directions that forecasts not only of wind and rain, but of freezing weather, are made with such

accuracy and expedition that farmers are supplied with warnings which enable them to take precautions which result in the prevention of much injury to their field crops and fruits. In the Division of Soils progress has been made with the electric method of moisture determination. "The work includes the record of evaporation to which the plant is subjected, the water supply maintained by the soil for supplying the loss due to this evaporation, and the intensity of the actinic and heat radiations which influence the physiological activities of the plant." The electrical method of salt determination in soils has proved of special value in areas which have been over-irrigated. The year's expenditure of the Agricultural Department amounted to the enormous total of over £480,000 sterling, and about one-fourth of this sum was spent upon the printing and circulation of agricultural literature. So great is the desire for information through this source that the supply is not equal to the demand.

Among the thirty-six special articles which are comprised in Part II. of this bulky volume, may be mentioned the following, which have more or less direct interest to readers in this country:—Some Types of American Agricultural Colleges, The Danger of introducing Noxious Animals and Birds, The Preparation and Use of Tuberculin, Pruning of Trees and other Plants, Utilising Surplus Fruit, Construction of good Country Roads, Grass Seed and its Impurities, and Notes on some English Farms and Farming. The book is beautifully illustrated with 42 full-page plates, and 136 figures in the letterpress.

R. WALLACE.

INHIBITED.

On Inhibition. By B. B. BREESE. *Psychological-Review*, iii., 1899: Monograph Supplement, No. 41, pp. 65.

The author gives a long account of a very elaborate series of experiments he has lately made to determine what conditions, both subjective and objective, affect binocular rivalry. He first gives an account of the views held in regard to inhibition by many psychologists, from Spinoza to Ladd. He concludes that these may be classified into five conceptions, the first four entirely psychical, and the fifth psychophysical.

"Almost universally," he says, "the instances of inhibition cited by the foregoing psychologists involve definite bodily activities, either within the field of sense perception or bodily movements. These instances fall under the following classes:—

1. Inhibition of one sensation by another: A faint sound is inhibited by a loud sound; a slight pain by a greater pain.
2. Inhibition of bodily movements by sensation: A sudden sight or sound may inhibit movements of walking, breathing or the action of the heart. Pain may inhibit the movements which cause it.
3. Motor activity may inhibit mental states: Activity in battle may inhibit fear. Motor activity inhibits the feelings of embarrassment. If, when trying to remember a name, some other name very similar is pronounced the first name is inhibited.
4. Emotions may inhibit bodily functions: Shame inhibits the action of the vasomoter muscles. Great dread inhibits the flow of saliva. Great grief inhibits the flow of the blood to the brain.
5. Will may inhibit the voluntary and half-voluntary movements of the body, and, to a certain extent, the involuntary muscles. Some people are able to decrease the activity of the heart at will.

Experimentally he has investigated two phases of inhibition within this field:—

(1) Inhibition of one sensation by another, and
(2) Inhibition of mental states through suppression of their motor elements."

He then fully describes his experiments, and thus summarises the results:—

"The length of time which the fields normally remain in consciousness was increased by direct will power. Efforts to decrease the number of changes of the fields in a given time were unsuccessful. With the so-called pure will efforts there were in every case accompanying eye movements. Elimination of the eye movements decreased the ability to hold either of the fields. The introduction of conscious eye movements was accompanied by a lengthening of the time of the field whose lines served as the guide for the movement. Counting the lines upon either field increased the length of time that field remained in consciousness. Figures which induced the greatest eye movement remained longest in consciousness. The lines of a moving field remained in consciousness nearly all the time, but did not inhibit the normal rivalry of the two fields. Contraction of the right side or of the left side of the body had the same effect upon the rivalry, viz., increased the time which the field before the right eye was seen. Coloured borders did not affect the rivalry. Of two fields of different sizes, the smaller remained longer in consciousness. Under different conditions adjacent parts of the retinae showed different rates of rivalry at the same time. Increase in the intensity of the light stimulus caused an increase in the rate of the changes, while the ratio of the phases of the rivalry was normal and constant. Of two unequally lighted fields, the lighter remained longer in consciousness. After-images showed the same phenomenon of rivalry; but the changes occurred at a slower rate than in the case of direct stimulation. When both fields were of the same colour the rivalry of the two sets of lines was not affected. Different stimuli falling upon the same area of the retina of one eye produced the phenomenon of rivalry.

He then treats of the "inhibition of motor reactions" and concludes by an endeavour to apply his results to education. He advocates strongly motor training. His most pertinent criticism of our prevalent methods of school education is the following:—"We imprison the child for hours each day in his seat; meantime we try to teach him to think without giving him a chance to react."

"From the point of view to which this work leads, the value of manual training for the development of the mind—*i.e.*, as a culture study—finds its basis in the very nature of consciousness. Here we find an explanation of the fact that the boy who gains the ability to perform bodily adjustments in a decided, accurate and rapid manner is better able to think accurately and clearly, and why a hesitating and inaffective bodily reaction is the accompaniment of a weakened or confused state of mind."

T. S. Clouston.

A LOYAL DARWINIAN.

Darwinism and Lamarckism, Old and New. Four Lectures. By Frederick Wollaston Hutton, F.R.S., etc. 8vo, pp. x. + 169. London: Duckworth & Co., 1899. Price 3s. 6d. net.

Captain Hutton's "excuse for adding to the already voluminous literature on Darwinism is that the subject is always advancing, and that the interest attached to it is not confined to naturalists, but enters into everyday life. It is, indeed, intimately connected with our systems of theology, for it forms one of the foundations—perhaps the corner stone—of Natural Religion. It is therefore important that a knowledge of the theory should be widely spread; and any attempt to convey that knowledge in simple language can hardly fail to do good, provided it be sufficiently clear to be understood at the first reading, and sufficiently short to discourage skipping."

But a new contribution to a subject so much over-written as this may perhaps be expected to justify itself by some particular quality, such as novelty of treatment, freshness of ideas, precision of statement, or up-to-dateness; but

we do not find this little book remarkable in any of these respects. It seems to us interesting rather as a clear exposition of the conclusions of one who began to write upon Darwinism in 1861, who has carefully examined many phases of evolutionary opinion, who remains after all a loyal Darwinian.

As one would expect from the author's varied contributions to natural history the book is saved by many concrete illustrations from seeming a merely logical discussion, and the exposition is on the whole delightfully clear, though it seems sanguine to hope that it will be altogether understood at the first reading. It requires some careful leading up before the reader can face with safety such a sentence as—"These indefinite variations may become definite through repetition; and are controlled in their development by the principle of selection, sometimes aided by use-inheritance."

As an old experienced hand, Captain Hutton is very careful in his use of terms, but occasionally his usage seems open to question. He speaks, for instance, of "the theory of development" contained in the "Origin of Species," but this phrase is more appropriately kept for the attempts to understand ontogeny. Similarly, when he says that "selection has no power if the individuals are not competing," he is either guilty of gross exaggeration or of an unjustifiable use of the word "compete," which seems almost irrelevant in those cases where the struggle is between the living creature and the inanimate environment. It seems to us also regrettable that the author does not take advantage of the distinction between modifications and variations which has been clearly defined and widely accepted, and saves a lot of time.

As to up-to-dateness, the book shows much of this quality, and yet not quite enough, for it is regrettable that suggestions like those in Weismann's "Germinal Selection," or in the so-called "Organic Selection Theory," should have been passed over in silence.

The first lecture on the scope and limitations of Darwinism is a fine illustration of successful exposition, to which personal reminiscences add interest. How many pages might have been saved—might still be saved—for more profitable use if critics would study Darwin's works as the author has done, or would even carefully acquaint themselves with a summary like this lecture. We need only recall Darwin's sentence—"Natural selection has no relation whatever to the primary cause of any modification of structure"—as a good instance of one of those so often forgotten.

The essence of the new Darwinism, according to the author, is found in the theory of isolation, which furnishes some sort of interpretation of the persistence of useless characters and incipient useful characters, and of the origin of divergence. A further difficulty—the existence of mutual sterility between different species—remains; but the author gets rid of it by saying:—"It has been shown to be outside Darwinism altogether; which is a theory of the preservation and development of variations, and not of their origin." As an example of the style, we may cite from this lecture the following passage:—"We may liken the progress of organic evolution to the march of an army, which is continually throwing off numerous scouting parties, who penetrate into every nook and cranny, and leave nothing unexplored. The few that find roads, lead off part of the army after them; while the majority, who fail to do so, perish on their tracks, and are heard of no more. Natural selection preserves and intensifies adaptations, or utilitarian characters only; isolation preserves both utilitarian and non-utilitarian characters. Progress is due to the former, variety to the latter." Thus the new Darwinism lifts us "out of the deadly region of utilitarianism into an altogether higher and purer air." Indeed, the air is so high and pure that we find it unsuitable for everyday respiration, for the author leads us to "the conclusion that all these so-called useless structures, all that give us beauty and variety, have been specially designed for man's education."

A condensed statement of the author's views would read somewhat as

follows:—He is willing to admit some use-inheritance or kinetogenesis, *e.g.* to explain the eye in flat-fishes and the tendrils of *Ampelopsis;* as panmixia cannot cause degeneracy and the principle of compensation of growth is an unproved hypothesis of a very doubtful character, disuse-inheritance seems to him necessary to explain many vestigial organs; environmental influence or physiogenesis is a true cause of variation, but these variations are not transmitted to other generations unless the same variation has been impressed over and over again on many successive generations; the most reasonable hypothesis appears to be that the physico-chemical forces affect, in time, the germ-cells; and that the changes thus produced become congenital variations, capable of being transmitted to future generations, and forming the material on which the various forms of selection and isolation may work.

We must not lay down this interesting book without noticing one of its most remarkable features, namely, the expression of the author's conviction that the outcome of the theory of evolution will be uniformity of religious belief.

J. A. T.

THE SCIENTIFIC SPIRIT.

Studien und Skizzen aus Naturwissenschaft und Philosophie. I. Ueber wissenschaftliches Denken und über populäre Wissenschaft. By Dr. Ad. Wagner. 8vo, pp. 79. Berlin: Gebrüder Borntraeger, 1899. Price 1 mark, 20 pfg.

This is the first of a series of booklets intended to introduce the reader to the problems of science and philosophy, not by didatic discourse or condensed summary, but by a more humane, indeed almost conversational, method. As an expert might tell us the meaning of the differential calculus in much less than half an hour, or of the theory of organic selection in five minutes, so will the author of these "Studien und Skizzen" instruct us concerning evolution and development, the freedom of the will and egoism, instinct and morals in a series of dainty little books which can be carried in the breast-pocket. It is a most laudable intention, and the prospect held out to us becomes the more enticing when we are told that the reader will be brought into touch with thought rather than with knowledge—in short with the scientific spirit rather than with the body of science.

The present volume deals with scientific thought—"wissenschaftliches Denken"—its aims and methods. It is easy to say—"the advancement of knowledge and the search after truth," but the conception of knowledge and truth seem to be as plastic as soft wax. "Tausend Gelehrte—tausend Ansichten." So much so that the public has become more or less consciously sceptical and shy of philosophy ("philosophiescheu"), and has fallen back into an intellectual slough which is called matter-of-factness. And even among the initiated the spectacle is seen of Philosophy receiving a pitiable alms at the door of the scientific mansion.

As a relief from this sluggish scepticism on the one hand and arrogant superficiality on the other, Dr. Wagner suggests that every man may be his own thinker. "Nur was selbst durchdacht ist, hat geistigen Wert . . . Immer und ewig ist die Parole: Selbst denken." This being granted, we are led by the author's lively conversation step by step to the conclusion—for which no novelty is claimed—that an unphilosophical science is a contradiction in terms, that there can be no wissenschaftliches Denken without a criticism of categories.

X.

AFRICAN FLORA.

Catalogue of the African Plants collected by Dr. Welwitsch. Vol. II. Part I. Monocotyledons and Gymnosperms. By A. B. RENDLE, M.A., D.Sc. 8vo, pp. 260. Printed by order of the Trustees of the British Museum. London, 1899.

The issue of this, the fourth, part of the Catalogue of Dr. Welwitsch's African plants within three years after the appearance of Part I., augurs well for the completion of the work in the near future. There is wanting to complete the account of the seed-plants only a few families of gamopetalous and the apetalous families of dicotyledons. This will presumably form a fourth and last part of vol. i. The work when finished will be a valuable contribution to our knowledge of the tropical African flora. No collector, however assiduous, collects everything, but examination of this and previously issued parts will show that Dr. Welwitsch obtained, during his eight years' stay in the country, not only a large number of species, but in most cases a good series of specimens illustrating geographical distribution of individual species. The account of his collections is therefore practically a Flora of that portion of West Tropical Africa which lies south of the equator. The district comprises Angola proper and the more southerly provinces of Huilla and Mossamedes, and the richness of the flora is evident from an analysis of the monocotyledons. All the African orders, comprising twenty-seven out of a total of thirty-four, are represented, and these include no less than 209 genera with 800 species. The most important are the orchids, with 18 genera and 76 species; Liliaceae, with 23 genera and 92 species; Cyperaceae, with 17 genera and 166 species; and grasses, with 75 genera and 268 species. Scitamineae, Amaryllideae, and Aroideae are also well represented. In striking contrast is the paucity of Gymnosperms. There are no Cycads and no Conifers, while the third order Gnetaceae is represented by a single endemic species of *Gnetum*, and that strangest of all seed-plants, the discovery of which we owe to Dr. Welwitsch, and which has hitherto been generally known as *Welwitschia mirabilis*. Unfortunately the rules of nomenclature will not allow this name to stand. It was proposed by Sir Joseph Hooker in honour of the discoverer in 1862, but exactly a year before a short notice had been published by Welwitsch himself, in which he suggested the name *Tumboa*, from the native name of the plant. So *Tumboa* it must be.

By the way, and the remark applies to the other parts which have appeared, we note with some regret the absence of plates. In the present part no less than 113 new species are described, and however full a description may be, there can be no two opinions as to the additional value of such plates as we are accustomed to associate with British Museum catalogues. A general account of the flora of the district in question would also form a useful appendix.

MODERN CHEMISTRY.

Grundriss der Allgemeinen Chemie. By W. OSTWALD. Third Edition. Pp. xvi. + 549, 57 figs. Leipzig: Engelmann, 1899. Price 16 marks, bound 17·20 marks.

This book, in its earlier editions, is well known, more particularly to the younger generation of students of chemistry. The first edition appeared in 1889, but in the ten years that have passed between that date and the appearance of this third edition, much new work has been done in the department of physical chemistry, or general chemistry as our author calls it. It is not the chemist alone who is indebted to Professor Ostwald for constituting himself the chief exponent of the newer views and their numerous applications, as he has

done in the present work and his other publications. New light is thrown by these views upon the operations of analytical, of organic, of technical, and in fact of all branches of chemistry; but the physicist and the physiologist will also find many obscure places rendered clearer when they become familiar with chemistry in its more recent physical development.

While the *Grundriss* is not a beginner's book, it is, relatively speaking, an elementary work, and it will serve to prepare the reader for the study of the same author's *Lehrbuch*, in which the subject is much more fully elaborated. In the present edition the book has been virtually re-written and is in many respects improved. Its appearance will be warmly welcomed by all who desire to see, and to assist in, the spread of the new chemical theories; and we are glad to think that the number of these persons is now rapidly increasing.

L. D.

GRADUS AD SCIENTIAM.

Progressive Lessons in Science. By A. Abbott, M.A., and Arthur Key, M.A. Pp. xi. + 320, with figures. London: Blackie and Son, Limited, 1899. Price 3s. 6d.

The first part of this book is an easy guide to a knowledge of the chemistry of air and water, and of such other portions of elementary chemistry as are considered requisite for an intelligent study of the second part. It is illustrated by means of simple experiments which, while not presenting any specially novel features, are, on the whole, well chosen; although they do not, in all cases, carry conviction regarding the conclusions intended to be drawn from them. The chapter on acids, bases, and salts can scarcely be regarded as satisfactory. The second part deals with the recognition, by chemical means, of the elements concerned in the building up of animal and plant tissues and with tracing these elements, generally, from the animal to the plant and from the plant to the soil. It may well be doubted whether this part does not demand too special a knowledge of certain very limited facts and methods of analytical chemistry to be of great use to pupils from a broad educational standpoint. The get-up of the book is good, and very few misprints have been met with. L. D.

VEGETARIANISM.

The Logic of Vegetarianism: Essays and Dialogues. By Henry S. Salt. 8vo, pp. 119. London: The Ideal Publishing Union, Limited, 1899. Price 1s.

In justification of the form of these essays, the reader has to bear steadily in mind that they were in the first instance published in *The Vegetarian*, and thus addressed to those already in sympathy with the writer's convictions. It would otherwise have been a serious tactical error to have personified his dialectic opponents under the titles he has selected. "Verbalist" and "Superior Person" may describe accurately enough one's idea of the mental condition of his adversaries, but they are not initiatory compliments such as smooth the course of an argument, and even "Patriot," when spoken with particular emphasis, may convey an irritating insult and cause much unhallowed rancour. What is perhaps more unfortunate from the critic's standpoint is that the mere use of these terms is in itself an argument which embodies a material fallacy described in text-books of logic under the heading of *petitio principii.* If, however, we pass these matters with a smile, much of the author's argument, especially on the ethical importance of food reform, will be found worthy of more than a passing thought. The weakness of the logical position of vegetarianism is, as Mr. Salt is fully aware, that its argument has to convince not reason but habit.

B.

VARIATIONS IN BUTTERFLIES.

Ueber einige Aberrationen von Papilio machaon. By Prof. J. W. Spengel. 48 pp. 3 pls. Jena: G. Fischer, 1899. Price 2 m. 50 pf.

Dr. Spengel's valuable paper on varieties of the common European "Swallowtail" Butterfly, which appeared in the *Zoologische Jahrbücher*, will be welcomed by entomologists in this separate form. After a careful description of the wing-markings and their position with regard to the nervures in typical examples of *P. machaon*, the author proceeds to an account of the various named aberrations which have been met with, his remarks being illustrated with excellent coloured figures. Specially noteworthy are the forms *evittata*, in which the black and blue sub-marginal bands are wanting; *nigrofasciata*, a melanistic form in which the red eye-spot of the hind wing tends to disappear; and *nigra*, in which all the wing-surfaces are suffused with black. Evidence is brought forward to show that the production of the melanic varieties does not depend necessarily on low temperature. The very remarkable form, *elunata*, is a monstrosity in which the wing-nervures are most imperfectly developed, they almost vanish towards the hind margin of the wing, and the sub-marginal dark band shows accordingly no segmentation. Dr. Spengel has materially advanced our knowledge of a fascinating subject.

G. H. C.

THE AFFINITIES OF THE TERMITES.

We have received the second and third parts of Mr. W. W. Froggatt's monograph of the Australian Termitidae (*Proc. Linn. Soc. N.S.W.*, 1896, 1897), comprising the general classification of the family and a detailed description of the known Australian species. In his discussion of the relationship of the Termites to other insects, Mr. Froggatt leans to the view that they have closer affinities to earwigs and cockroaches than to any other group, and that they should therefore be included among the Orthoptera rather than among the "Pseudo-neuroptera."

AUSTRALIAN ECONOMIC ENTOMOLOGY.

Mr. Froggatt is also devoting attention to injurious insects in New South Wales. A paper by him on "Gall-producing Insects," with special reference to Coccids, is published in the *Agricultural Gazette*, *N.S.W.*, 1898, while in conjunction with Messrs. Allen, Blunno, and Guthrie, he has issued an excellent illustrated pamphlet on "Insect and Fungus Diseases of Fruit-trees." The various pests are grouped according to the trees which they injure. Each species is clearly figured, and the best means for clearing the orchards is plainly described.

DIARY OF TWO ORNITHOLOGISTS.

Bird Life in an Arctic Spring. The Diaries of Dan Meinertzaghen and R. P. Hornby. Crown 8vo, pp. 150. London: R. H. Porter, 1899.

This dainty little volume has been published as a memorial of Dan Meinertzhagen, who recently succumbed to a brief illness at the early age of twenty-three. He was always devoted to birds, and had made a special study of the Raptores, upon which he hoped to complete a Monograph. But the material which has found its way into print is a literal transcript of a private journal kept during a visit paid to Finland in the summer of 1897, supplemented by the notes of the young sportsman who shared his hardships. The

diaries of the friends were written up as occasion permitted, often at the end of a long fatiguing day, and cannot be said to do more than sketch the bird life to be found in the forests of Northern Europe; but they are vivacious, and have the merit of severe accuracy. While many of the birds that inhabited the neighbourhood of Muonioniska proved to be species that can be studied in the British Islands, such as the Capercaillie, Osprey and Merlin, others were characteristic of the far north, such as the Lapp Owl, Pine Grosbeak and Siberian Jay. Dan Meinertzhagen was an accomplished draughtsman as well as a good naturalist, and he found time to make some capital sketches of birds that he encountered, *e.g.* that of the Hawk Owl which is reproduced at p. 74. Had his life been spared for a few years, he might well have ranked as one of the first zoological painters of the day. The twenty-seven plates bound up at the end of the volume show the pains which he had taken to master the technique of his art. Perhaps he excelled most in delineating the attitudes of birds of prey; but he was also adept in preparing drawings of anatomical dissections. The feeling of regret which all readers of "Bird Life in an Arctic Spring" will experience, after perusing the story of a life of brilliant promise suddenly cut short, is deepened by the knowledge of the amiable disposition of this ardent naturalist, who readily won the regard of all with whom he came into contact. The fresh and vivid impressions of Arctic bird life which his rough jottings convey may well inspire others to follow in the wake of his investigations. H. A. M.

RENAL SECRETION.

Les Fonctions Rénales. By Prof. FRENKEL of Toulouse. Pp. 84. [Scientia.] Paris: Georges Carré and C. Naud. 1899. Price 2 francs.

In this little book] of eighty-four pages, Prof. Frenkel has given a very interesting account of the physiology and pathology of renal secretion. In the first chapter there is a short but well-written description of the structure of the kidney, and this is followed by one dealing with the composition of the urine, in which the biological properties of the latter are specially emphasised. As one would naturally expect, seeing that the work has been largely done by French scientists (Bouchard, Charrin), a much larger amount of space is devoted to the toxicity of the urine than is ordinarily met with even in far more ambitious text-books in other languages. Although many of the hypotheses, which the author formulates in regard to the properties of the urine, may be considered to have insufficient basis, all must admit that the author has stated his case clearly. The third chapter, on the physiology of renal secretion, goes over well-known ground, the theories of Ludwig and Bowman-Heidenhain being shortly referred to; but recent English work is not mentioned. The fourth chapter deals with a department with which the names of Brown-Séquard, Teissier, and the author are associated, viz. the nature of internal renal secretions. In this country and in Germany, much more attention has been paid to the secretions of the pancreas, thyroid and supra-renals, than to renal secretions. The last two chapters treat respectively of what the author terms, pathological physiology of the renal secretion and renal permeability and insufficiency. The little book may be heartily recommended to all interested in this subject. T. H. MILROY.

The *American Naturalist* for September has the following articles:—"A Contribution to the Life-History of *Autodax lugubris* Hallow, a Californian Salamander," by W. E. Ritter and Love Miller; "The Worcester Natural History Society," by H. D. Braman; "Synopsis of North American Caridea," by J. S. Kingsley; "The Life Habits of *Polypterus*," by N. R. Harrington; and "Pads on the Palm and Sole of the Human Foetus," by R. H. Johnson.

The *Halifax Naturalist* for October has the following contents:—"Scraps of the Life-History of Insects," by Miss Theodora Smith; "The Life-History of the Autumn Crocus," by C. E. Moss; "Moorland Moths," by E. Halliday; "Haugh End," by J. Longbottom; "The Flora of Halifax," by W. B. Crump; Field work in winter; and Notes.

The *Naturalist* for October has papers on "Air Blasts below Ground," by H. Preston; on "Botanical Finds in Cumberland," by W. Hodgson; on "Nottinghamshire Diptera," by Rev. A. Thornley, and on the "Florula of Bare, West Lancaster," by F. Arnold Lees.

The Canadian Record of Science, after eight months' delay, brought out the first number of its eighth volume on 1st September. The publication committee of the Natural History Society of Montreal hopes in future to present the *Record* regularly each quarter. The number before us contains a paper by Professor E. W. M'Bride on "Zoological Problems for the Natural History Society of Montreal," much of which is applicable to other societies of the kind. The main contention is that, when once the local society has compiled complete lists of the local fauna and flora, the attention of the naturalists should be directed to the study of each species in relation to its environment. How far are the distinctive characters of a species concordant with its special habits? What prevents two species living side by side from intermingling? How far have the species of the systematists a physiological validity?

Other papers are a list of "The Gramineae, Cyperaceae and Juncaceae of Montreal Island," by Harold B. Cushing and Robert Campbell, and "Dimorphism and Polymorphism in Butterflies," by H. H. Lyman.

The study of small mammals has, as many other studies in zoology, arrived at a stage where the chief desideratum is enormous quantities of individuals for purposes of minute comparison. At the same time the study is so refined that the specimens, to be of service, must all be prepared in a similar manner. Those who are willing to help specialists by collecting for them will be glad to have instructions clearly and compactly placed before them; and this they can now find in a pamphlet entitled "Directions for Preparing Study Specimens of Small Mammals," issued by Gerrit S. Miller, Jun., as Part IV. of *Bulletin of the U. S. National Museum*, No. 39 (10 pp. Washington, 1899). Special hints for tropical climates are furnished by E. W. Nelson, the well-known collector.

Knowledge for October is a strong number,—Professor Arthur Thomson of Oxford discusses "Cranial form"; Sir Michael Foster's presidential address at Dover is expounded; H. F. Witherby continues his account of two months' natural history on the Guadalquiver; Mr. Stebbing continues his wonderful story of the Karkinokosm; Mr. W. S. Bruce, lately returned from a cruise with the Prince of Monaco, deals graphically with a haunt of his—the top of Ben Nevis; Mr. J. E. Gore still discourses with interest on some suspected variable stars, and Prof. Cole introduces the reader to the secret of the Great Earth-mill, and there is more besides.

The *Quarterly Review*, No. 380, published on the 18th October, has an illustrated article on "The Penycuik Experiments," which were discussed in our last volume.

The *Journal of School Geography* for September has the following articles:—"Equipment of a Meteorological Laboratory," by R. De C. Ward; the Earth's Interior," by J. A. Bownocker, "Niagara Falls, and the Commerce of the Great Lakes," by C. A. M'Murry, and "the Caroline Islands."

We have received a number of agricultural papers of interest, though dealing with matters somewhat beyond our scope. From the Department of Agriculture in the University of Aberdeen comes a report on an investigation with

regard to the value of tuberculin as a test of the presence of tuberculosis in cattle, by J. M'Lauchlan Young, F.R.C.V.S. and Dr. J. S. H. Walker. The numerous and clearly displayed statistics show that when used with care and under proper conditions tuberculin is a reliable diagnostic of tuberculosis in cattle, except (*a*) when the tubercular lesion is minute, or (*b*) when the disease has become generalised, especially in the case of aged and emaciated animals. Two other conclusions reached are that tuberculin (as has been previously pointed out) loses its virulence when kept for a time, and that tuberculous udders are more frequent than is generally believed to be the case.

From the Department of Agriculture of New Zealand comes a report on swine-fever by J. A. Gilruth, M.R.C.V.S., chief government veterinarian and bacteriologist, in which it is shown that pulmonary and pleural lesions may, and frequently do, occur along with, or independently of, the so-called bowel-lesions of swine-fever. It is doubtful if the hog-cholera and the swine plague of America are two distinct diseases as they are reported to be. It is possible that as the thoracic lesions of swine-fever seem to be the more frequent and more marked phenomena of the disease, this may be the key to the non-success of the stamping-out order in Britain, which only takes account of the gut-lesions, not to mention that the virulence of the disease seems to increase and diminish from unknown causes—an unfortunately necessary lame ending to the report.

The *Irish Naturalist* for October contains the following short papers:—"Some Animals from the Macgillicuddy's Reeks," by R. F. Scharff and G. H. Carpenter; "Migratory Butterflies in S. W. Cork," by J. J. Wolfe; "*Matricaria discoidea* in W. Ireland," by N. Colgan, with a note by C. Lloyd Praeger; "*Poa compressa* as an Irish Plant," by J. H. Davies; and Notes.

The *Scientific American* for Sept. 23 has an article on women in science, based upon a recent work by Rebière, in which the rôle of honour is traced from Hypatia onwards to Sofia Pereyaslawszewa, and indeed to our midst. The fact is that to recognise the sex-distinction in scientific work is now almost an impertinence.

In *Nature Notes* for October, besides the usual Selborniana characterised by sensible humanitarianism, there are "Observations on the Origin and Dispersal of Fruits and Seeds," articles on the regeneration of the New Forest, by a Selbornian, and on batrachians as pets by G. Renshaw, and other interesting matter.

The fourth number of *L'Anthropologie* for 1899 contains *inter alia* a re-discussion of polymasty and polythely in man by Dr. P. J. Stoyanov.

In *Science Gossip* for October there are, besides continued articles, various short papers:—"A Heronry in Asia Minor," by J. Bliss. "Irish Plant Names," by J. H. Barbour; "Radiography" (with figure of a rabbit's fore parts), by J. Quick; "Manganese in River Gravels," by M. A. C. Hinton; "The Birch and the Alder," by Dr. Keegan; and "Larvae of Caprella," by E. H. Robertson.

The *Scientific American* for September 9 republishes Mr. Lydekker's article on "A Contrast in Noses," for which they are indebted to *Knowledge*.

We have received from the University Corresponding College Press "The London University Guide for the year 1899-1900," which bears a protective resemblance to a University Calendar, and is full of valuable information for intending students.

Messrs. Clay, Cambridge University Press, announce the fourth part of Dr. Willey's "Zoological Results," Parts I. and II. of the second volume of "Fauna Hawaiiensis," and the second volume of Mr. Seward's "Fossil Plants."

OBITUARY.

GEORGE DOWKER.

Born, April 2, 1828; Died, September 22, 1899.

Kent has to deplore the death of one of her foremost geologists, botanists, and archaeologists. Mr. Dowker had only returned from the meeting of the British Association a few hours before his death, which occurred quite suddenly at his home in Ramsgate. Born at Stourmouth House, Stourmouth, he was educated at Sandwich Grammar School, and trained for agriculture at Hodsdon Agricultural College. He began farming his father's estate at the early age of 30, but science claimed too much of his time to allow of his success. As a botanist, Dowker was the authority on Kentish plants, many of the rarer species in the *Flora of Kent*, edited by Hanbury and Marshall, being associated with his name. As geologist, he was responsible for numerous papers, notably "On the Chalk of Thanet," and "On the Water Supply of East Kent." As a microscopist, he was acquainted with the pond life of his district, and at one time was president of the Margate Microscopical Club. As archaeologist, he contributed to *Archologia Cantiana* many valuable papers on Richborough Castle, The Reculvers, and Roman antiquities at Wingham, Preston, and other places, and he it was who described the Saxon Cemetery at Wickhambreaux.

Dowker's collection of chalk flints is now in the Maidstone Museum, but he leaves behind an excellent herbarium of wild plants. He was buried at Stourmouth, and Kent has lost a devoted and earnest student of a class only too rare in Thanet.

The following deaths have been recently announced:—Grant Allen, *facile princeps* as an exponent of evolutionary natural history, on Oct. 25, in his 51st year; Prof. Max Barth, director of the agricultural experiment station in Rufach (Alsace) on August 28, in his 44th year; on October 6, at the age of 63, John Bridgman, entomologist and a vice-president of the Norfolk and Norwich Naturalists' Society—he had presented his collections to the Norwich Museum; Sigismondo Brogi, a well-known naturalist in Siena, on July 17, at the age of 48; Dr. Karl Bernhard Brühl, formerly professor of zootomy in the University of Vienna, on August 14, in Graz, at the age of 79; J. B. Carnoy, professor of botany in the Catholic University of Louvain, editor of *La Cellule*, well known for his researches on cell-structure and on the phenomena of maturation and fertilisation, on September 8, during a holiday in Switzerland, 63 years of age; Chief-Justice C. P. Daly, at the age of 84, for many years president of the American Geographical Society, to which he rendered great services, also an enthusiastic botanist and one of the managers of the Botanical Garden of New York; Prof. Theodor Elbert, geologist in Berlin, at Gross-Lichterfelde, in his 42nd year; Prof. Joseph Erhardt, formerly director of the Natural History Museum in the Castle at Koburg, in his 80th year; Dr. W. D. Hartman, conchyliologist, in West Chester, Pa., on August 16; at Geneva, Hippolyte Lucas, entomological assistant in the Paris Museum of Natural History; on September 29, Dr. C. Russ, ornithologist, of Berlin; Julius Scharlock, an enthusiastic florist, at Graudenz, on August 14, in his 90th year; Christian Schwemmer, botanist in Nürnberg; Dr. Friedrich Theile, author of several publications on natural science, at Lockwitz, near Dresden, in his 85th year; Gaston Tissandier, founder and editor of the scientific weekly, *La Nature*, in Paris, on September 8, aged 56; Rev. William Farren White, entomologist, on July 21, at Bournemouth, in his 66th year.

CORRESPONDENCE.

RAINFALL.

Dear Sir—Without being able to positively assert that the statement on p. 308 of *Natural Science* (October) that 351 inches of rain fall in N. England and Scotland, and that London has ten times the rainfall of Paris, is completely wrong, I believe it to be incorrect. At the Stye, near Seathwaite, Cumberland, the average is 177 inches ("Bartholomew's Physical Atlas," vol. iii. Meteorology, p. 20); at Ben Nevis 151 inches. In De Lapparent's *Leçons de Géographic Physique* on p. 65, map Repartition des Pluies en France, Paris occupies an area with 500 to 600 mm. rainfall = 20 to 24 inches, and in the atlas above mentioned (p. 22), Paris is said to have 20·7 inches of rainfall, that of London (p. 24), being 20 inches at Crossness, E.—Yours truly, Bernard Hobson.

Owens College, Manchester,
Oct. 10, 1899.

CAPE FISHERIES.

Dear Sir—Owing to absence from home my attention has only just been called to the letter by Dr. Gilchrist in your issue of September and to your editorial note below it.

I desire to say that I have not in any way either misunderstood Dr. Gilchrist or misinterpreted him, and must entirely dissociate myself from your note. I think that had the facts been placed fully before you the latter would not have been written.

Let us briefly recall the facts under discussion. In *Natural Science* for June (p. 431) I wrote:—"Dr. Gilchrist, the Government Marine Biologist, states in evidence that 'we know absolutely nothing about the spawn of the fish.' This statement seems to require some explanation, considering that the author of it has been over three years in Cape waters, and that an annual expenditure of 'between £3000 and £4000' has been placed at his disposal."

My justification for these remarks is to be found in the "Minutes of Evidence taken before the Select Committee on the Fishing Industry," contained in the Report under review.

Dr. Gilchrist in reply to Q. 658 by Mr. Maasdorp, after referring to the condition of affairs in British waters, says:—"The question here is very different. The evidence has shown that we know absolutely nothing about the spawn of the fish, or very little; we do not know whether it floats on the surface of the sea or whether it lies on the bottom. Some spawn has been seen, but it has not been identified to what fish it belongs" (p. 61).

The ordinary intellect would suppose that the meaning of those remarks is to the effect that Dr. Gilchrist agreed with "the evidence" and endorsed it. He cites the evidence himself and follows it up by remarks of his own agreeing

with it. All possible room for prevarication is removed by Dr. Gilchrist's answer to Q. 724. Here it is—

"724. Is anything definite really known about the spawn of our South African fish?—Nothing. Several people have seen the spawn of some species of fish. It is said to discolour the sea for many miles" (p. 68).

In view of such evidence as this I fail to see the point of Dr. Gilchrist's letter, in which he implies that it was only the fishermen who stated that nothing was known about the spawn, and that he had facts up his sleeve bearing upon the question, or at least had not confessed his ignorance to the Commission.

When we recall to mind the conditions under which the spawning habits of our British fishes have been investigated, by the help of small sailing vessels or excursions on commercial trawlers, we feel quite justified in our remarks. The *Pieter Faure* appears to have been free to trawl whenever and wherever was most desirable, and we have only to conclude that for three breeding seasons she has caught thousands of fish which must have been in various stages of maturity, yet at the end of this time the scientific expert has to own that nothing definite is really known about the spawn of our South African fish. Doubtless there may be a good enough explanation forthcoming, but Dr. Gilchrist's letter does not shed any further light upon the matter.

We may take Dr. Gilchrist's word for it, in answer to the Chairman, that the *Pieter Faure* has not been used for "picnics and pleasure-trips," and can quite believe that the "people who came for picnics did not find it very agreeable" (Q. 680, p. 65), for the deck of a trawler is not an ideal place for such proceedings, but we feel that if more of the scientific results of the trawling had been produced on evidence there would have been no occasion for such a question.

The only other remark I made was that "the scientific voice seems to lack decisiveness." Here perhaps the term "accuracy" would have been preferable to "decisiveness," for a very few days' sojourn in any of our marine laboratories should have been sufficient to convince Dr. Gilchrist that it is not a "*theory* that the spawn of most fish" floats at the surface (Q. 725), but a *fact* capable of easy demonstration, and, secondly, that "the herring is" *not* "about the only fish known to spawn on the ground" (Q. 726).

From the above I trust it is clear that, although I may seem to have misunderstood Dr. Gilchrist, in this case things are not what they seem.—Yours, etc.,

THE REVIEWER.

Sept. 25, 1899.

CORRIGENDA.

Since the address affixed to the MS. of the "The Fauna of the Sound" (*Nat. Sci.* xv. pp. 263-273) escaped your notice, the proofs of that paper never reached either me or Dr. Lönnberg. The following corrections will bring the published pages into better agreement with the original MS.:—

P. 263, line 1, *for* nörande *read* rörande.
P. 263, line 2 from bottom, indenfor is one word.
P. 267, line 5 from bottom, *for* Shore-regions *read* Shore-region.
P. 269, 271, 272, *for* Bohustän *read* Bohuslän.
P. 266, in the italicised line, *for* Oscidians *read* Ascidians.

F. A. BATHER.

NEWS.

The following appointments have recently been made :—Dr. Ardaillon as professor of geography in the University of Lille; Marshall A. Barber, as associate professor of cryptogamic botany in the University of Kansas; Dr. G. A. Bates, as professor of histology in Tufts College Dental School; R. K. Beattie, as instructor in botany in the Agricultural College at Pullman, Washington; H. M. Benedict, as head of the biological department of the Nebraska State Normal School at Peru; Dr. R. M. Buchanan, as bacteriologist to the City of Glasgow; Judson F. Clark, as an assistant in botany in Cornell University; Dr. E. D. Copeland, to be assistant professor of botany at the University of West Virginia; Dr. Karl Josef Erich Correns, as titular professor of botany in the University of Tübingen; Dr. Deichmüller, of the mineralogical and ethnological museum in Dresden, to the title of professor; Dr. Julius Doeger as adjunct to the Austrian Geological Survey; Dr. W. Figdor to be privat docent in botanical anatomy and physiology in the University of Vienna; F. P. Gorham, to be assistant professor of biology at Brown University, U.S.A.; H. Hasselbring, as an assistant in botany at Cornell University; George J. Hastings, as an assistant in botany at Cornell University; Dr. Max Hollrung, director of the Experiment Station for plant protection at Halle, to be titular professor; Alfred Jentzsch, to be geologist at the Geological Landesanstalt in Berlin, in succession to the late Professor Th. Ebert; Dr. J. B. Johnston, to be assistant professor of zoology at the University of West Virginia; J. L. Kellogg, as assistant professor of biology at Williams College, Williamstown, Mass.; Dr. F. D. Lambert, instructor in biology in Tufts College, U.S.A.; Albert B. Lewis, assistant instructor in zoology in the University of Nebraska; Miss Annie Lyons, as assistant in zoology at Smith College, U.S.A.; Harold Lyon, assistant in botany in the University of Minnesota; C. B. Morrey, to be assistant professor of anatomy and physiology at the Ohio State University; Dr. Elisa Norsa, assistant in zoology in the University of Bologna; Karl Reinhertz, as professor of geodesy in the Technical Institute in Hannover; P. H. Rolfs, as professor of botany at Clemson College, and botanist to the Agricultural Experiment Station of South Carolina; William Norman Sands, as director of the botanical station in Antigua; J. L. Sheldon, instructor in biology in the Nebraska State Normal School; Dr. Max Standfuss, as titular professor of zoology in the University of Zürich; Dr. F. E. Suess, assistant on the Austrian Geological Survey; Dr. F. Supino, to be assistant in the Zoological Institute of the University of Rome; R. W. Tower, to be assistant professor of chemical physiology at Brown University; Mr. W. H. Twelvetrees, as geologist to the Government of Tasmania; F. E. Watson, graduate assistant in zoology in the University of Nebraska; Professor W. M. Wheeler of Chicago, as professor of zoology in the University of Texas; W. H. Wheeler, assistant in botany in the University of Minnesota; Dr. A. Willey, as lecturer on biology at Guy's Hospital, London; R. H. Wolcott, as adjunct professor of zoology in the University of Nebraska; Ernst Anton Wülfing, as professor of geology and mineralogy at the Agricultural Institute in Hohenheim.

We learn from *Science* that Dr. R. Burckhardt, professor of palaeontology at Bâsle, and Dr. V. Uhlig, professor of geology in the German Technical Institute of Prag, have been elected members of the Academy of Sciences of Halle.

Professors D. J. Cunningham and W. C. M'Intosh have been appointed as scientific members of a commission to inquire into inland fisheries in Ireland.

Ernst Ebermayer resigns his professorship of forestry in the University of München.

W. von Ahles resigns his professorship of botany in the Technical Institute in Stuttgart.

A bacteriologist is wanted for the Glamorgan County Council and Cardiff Corporation, who shall also lecture on bacteriology in the University College, Cardiff. The salary is £300. Applications have to be sent before 6th November to Mr. W. E. R. Allen, County Offices, Cardiff.

During October the Swiney Lectures on Geology in connection with the British Museum (Nat. Hist.) were delivered by Dr. R. H. Traquair, who chose for his subject the "Pleistocene Mammalia." Since the Natural History Museum is still without a lecture-theatre, the course was given at the Museum of Practical Geology. After next year, when Dr. Traquair's appointment terminates, the post will be open to any doctor of medicine or science of the University of Edinburgh.

A curious thing about this lecturing at Jermyn Street is that the audience is far smaller than it used to be at South Kensington. However convenient Jermyn Street may be for the agriculturist in search of water, or the mining speculator who wants an analysis of a new sample of ore, it is, in the opinion of the Swiney lecturer, more remote from the ordinary student of natural science than is South Kensington. This tells against those who wish to keep all the Survey collections in their present confined quarters.

A circular from an influential committee formed at the Dover meeting of the British Association is headed with the words: "It is at least probable that the closing year of the nineteenth century, in which science has played so great a part, may, at Paris, during the great World's Fair—which every friend, not of science only, but of humanity, trusts may not be put aside or even injured through any untoward event, and which promises to be an occasion not of pleasurable sight-seeing only, but also, by its International Congresses, of international communing in the search for truth—witness the first select Witenagemote of the Science of the world."

It proceeds to say that, "Following upon the hopes and counsels of Sir Michael Foster's Presidential Address and upon the reunions of the British and French Associations, it is felt that the time is now ripe for some more permanent organisation which should maintain, develop, and utilise the good relations thus so fully initiated. It is therefore proposed to form a General and Advisory Committee consisting of members of the British Association, the Association Française, and of other representatives of Pure and Applied Science, Education, Art, etc., with the object of promoting arrangements for an International Meeting or Assembly in connection with the Paris Exposition of 1900."

"It is widely felt that there is not only room but need for some organisation which would bring together, for each of the leading Departments and Congresses of the Exposition, the specialist, the educationalist, and the intelligent public; and this on all grounds, from those of personal convenience, and economy of time, money, and effort, to the highest considerations of scientific progress and international amity."

Names of those willing to join the General Committee of this proposed Paris International Assembly, which, we learn, has been warmly welcomed in France, and has received a munificent beginning to a guarantee fund in Britain,

should be sent to Profs. MAVOR and GEDDES, Acting Secretaries, T. R. MARR, Assistant Secretary, 5 Old Queen Street, Westminster, London, S.W., or 95 Boulevard St. Michel, Paris.

At the Paris Exposition there is also to be an International Congress of Physics, in regard to which a prospectus has been issued.

We learn from the *Scientific American* that an aquarium will be among the attractions at the Paris Exposition. "A dark incline will lead visitors to it, and suddenly they will feel as if transported to the very bottom of the sea, in the midst of marine landscapes and inhabitants of the ocean."

At a meeting held in The Outlook Tower, Castlehill, Edinburgh, on 14th October, Prof. James Geikie, D.C.L., in the chair, an interesting and stimulating address was delivered by Prof. Wilbur Jackman, M.A., of Chicago University and Training College, on "Nature-Study, its Methods and Results in School Practice." Even apart from the able address, which will doubtless be published, the exhibits of notes of work, especially those in water-colour, arranged round the room, showed what results await those teachers who have the courage and opportunity to devise courses of nature-study to mitigate the burden of book-work. To many of those present these exhibits and the story of them must have seemed a revelation, but it was interesting to notice that several authorities who took part in the discussion, which lasted for towards two hours after the lecture, reverted to the necessity of "books." A guide-book for the teacher may be necessary—not that there is really a lack—but of more books for the scholars there should, in a case like this, be no mention. Owing to the overcrowded audience, an adjournment after the lecture was effected to the Castlehill public school, where, under the chairmanship of Prof. Crum Brown, F.R.S., an interesting discussion was held. To this contributions were made by Mr. Robert Smith, B.Sc., of University College, Dundee, who reported on some nature-study classes which he had conducted, by Mr. Robert Blair, Science Inspector, by Dr. Dunn, H.M.I.S., by Prof. J. Arthur Thomson of Aberdeen, by Dr. Maurice Paterson of the Free Church Training College, by Miss Stevenson of the Edinburgh School Board, by Mr. Walter Blaikie, Prof. Geddes of Dundee, and others.

There was also an exhibition of maps of a botanical survey of Scotland by Mr. Robert Smith, of a cosmosphere by Mr. Walter Blaikie, of a first panel of a proposed spheric atlas by Prof. E. Reclus of Brussels, of relief models by Mr. George Guyou, etc. Altogether the meeting was one of considerable educational importance in connection with the teaching of natural science in schools.

In connection with the problem which the recent codifying of "nature-study" has raised, we would be frank in remarking that "nature-study" is as difficult as it is valuable as an educational discipline, and that, the facts being as they are, the further education of the teachers (and their better remuneration, which is an obvious correlative condition) must be recognised as indispensable to success. Badly taught spelling may be bad, but it hardly affects morals; badly taught grammar may be worse, but it is rarely forcible enough to warp the outlook of a lifetime; but badly taught science by incompetent teachers is probably worse than none at all. We know full well that there are many splendidly equipped science teachers in our primary schools throughout the country, but this is certainly not, necessarily not, the case with most. To overhear a class repeating "the stomach is a bag at the end of the alimentary canal" would suffice to show even cranks for science teaching that the seamy side is distressfully ragged. And we may quote another illustration, supported by an editorial comment in our successful contemporary *The American Naturalist* for September, in which it is pointed out that *The Great Round World*, an excellent juvenile newspaper, tells the child audience that a Siberian traveller has found a beautiful flower that blossoms in January, resembling the Convolvulus, a

blossom lasting only a day, and that on the third or fourth day it has the ends of the fine anthers tipped with glistening diamond-like specks—the seeds. The *seeds, parbleu!* And this is called "Easy Science."

We learn from the very excellent September number of the *Journal of Applied Microscopy* that the Marine Biological Laboratory at Wood's Holl has just closed its twelfth annual session. The year has been a very successful one, additional courses were offered, attendance considerably increased, and a deep interest manifested. It is the purpose of the management to further broaden the scope of work. A thorough course in nature-study will be introduced next year. An addition to the botanical building and a new building for research laboratories are also expected.

The Natural History Society of Montreal has issued an appeal for financial aid. This has been rendered necessary by the discontinuance of the grant from the Quebec Government, which used to defray the cost of publishing *The Canadian Record of Science.* The Society does good work in maintaining a library and museum, the latter open free on Saturdays and on Wednesday afternoons and visited by 4000 people during the past year. Under the auspices of the Society, Saturday half-hour lectures to young people are delivered, as well as the Somerville lectures to grown-ups. The number of members is only about 170. We hope the Society will receive the support it deserves.

At the annual meeting of the Hull Scientific and Field Naturalists' Club, held on 20th September, it was stated that 54 new members had been elected during the past year, raising the membership to 165. At the fortnightly meetings during the year lectures were delivered, several dealing with local natural history. Sectional meetings were also held, and at them practical demonstrations were given by the recorders and other officers of the club. During the summer months field meetings were held as usual, and excursions made to places in the neighbourhood. By the publication of *Transactions* (previously noticed by us) the Society has been able to add to its library. A microscope club has been started to enable members to buy microscopes at reduced rates. The President for the ensuing year is Mr. R. H. Philip. The Secretaryship remains in the able hands of Mr. Thos. Sheppard, 78 Sherburn Street, Hull.

In *Science* for 29th September there are some interesting notes by "F. A. L." on "The Work of Foreign Museums." The Australian Museum leads the list in expenditure, though this only amounts to $35,000; the Colombo Museum, the official museum of Ceylon, had 111,000 visitors in 1898, and yet suffers for lack of funds and paint; the activity of the museum at Prag is shown by the numerous meetings of the association by which it is controlled and by its important publications, *e.g.* Fric's *Fauna der Gaskohle;* the West Prussian Provincial Museum is very strictly regional; the Norwich Museum likewise; the Manchester Museum is "a very live museum," but this is hardly "news."

From the Report of the Australian Museum for 1898 we glean the following information:—Few purchases were made, owing to want of funds; on the other hand, a circular appeal for objects illustrating Australian ethnology has met with a gratifying response. A large collection of miscellaneous objects from Pacific islands has been presented by Rev. S. Ella. The Rev. H. A. Robertson of Erromanga, New Hebrides, has presented a cooking-pot and two large stone rings, known as Navilah or moon-rings, of great rarity and value. There have been purchased a remarkable inlaid skull from the Solomon Islands, and a valuable series of objects from Thio, New Caledonia, including two funeral masks, shell money, and a *doigtier* or spear-thrower. In this department all the unexhibited specimens have been arranged systematically, and the phallic specimens, of which the Museum possesses a fine series, have been arranged in a private room and labelled. The zoological collections have been enriched by many specimens from the Zoological Society of New South Wales,

including a donkey brought back from the Soudan in 1885 by the New South Wales Infantry. Prof. W. B. Spencer presented several specimens of Central Australian Muridae. Mr. Waite is making a card catalogue of the mammals, and finds the plan exceedingly convenient. A new spirit house has been built, and thousands of specimens in spirit have been safely transferred to it. The detrimental practice of keeping birds' skins in spirits has now been stopped. The skeleton of a large sunfish, *Orthagoriscus mola*, is being prepared by the method used for cartilaginous skeletons. It is worthy of note that the exhibited shells have to be protected by movable covers, since their colours are bleached by the strong light. The Tunicata of New South Wales have been studied by Prof. Herdman, who has compiled a "Descriptive Catalogue of the Tunicata in the Australian Museum, Sydney, N.S.W.," printed in Liverpool, and published about midsummer last. This gives to the Museum some fifty types. Under Palaeontology it is stated that Mr. C. W. de Vis of the Queensland Museum has continued the determination of the extinct marsupial remains. The more important donations were: Mesozoic, Carboniferous, and Silurian fossils of Tasmania, by T. Stephens; Cretaceous reptilian and fish remains from the Flinders river, by J. B. Nutting; and Prof. R. Tate's co-types of Ordovician fossils from Central Australia, by W. A. Horn. The collection of meteorites has been added to by casts, slices, and a small iron meteorite from West Australia. Many Australian minerals have been presented, and among them a fine series of native copper from Broken Hill. These excerpts by no means exhaust the interest of the Report. The amount of work done under discouraging circumstances is highly creditable to the staff. It is clear they do not go to sleep, for sixteen telephones have been distributed throughout the building, "and have already proved a source of great convenience and saving of time."

Science notes that the last report of the Royal Zoological Society of Amsterdam commemorates the sixtieth year of its existence. Besides the well-known zoological garden, the Society maintains a fine aquarium, a zoological museum, a geological and palaeontological collection, a library, etc., a combination which affords fine facilities for scientific work. It will be remembered that Fürbringer's monumental work on the morphology of birds was among the publications of this Society.

On September 11, Alderman George Collard, Mayor of Canterbury, opened in that town a new institute, library, and museum, in great part the gift of the late Dr. Beaney of Melbourne.

We learn from *Nature* that a commencement has been made with the new Geological Museum at Oxford. The Museum will cost about £44,000, the fund raised at a memorial to Prof. Sedgwick supplying £27,000.

Science for September 22 quotes from the report of the Australian Museum for 1897 an interesting observation in regard to a specimen of the Galapagos tortoise, *Testudo nigrita*, brought to Sydney in 1853. It then weighed 53 pounds, while at the time of its death, in 1896, its weight had increased to 368 pounds, "a more rapid rate of growth than such animals are usually credited with." It is now mounted in the Museum.

Science reports the following gifts and bequests:—$300,000 given by Mr. Edward Tuck of New York to Dartmouth College; $60,000 bequeathed by Mrs. Mary D. Goddard to Tufts College; $10,000 bequeathed by Richard B. Westbrook of Philadelphia to the Wagner Institute of Science, to endow a lectureship for "the full and fearless discussion by the most learned and distinguished men and women in our own and other countries of mooted or disputed questions in science, and especially the theories of evolution."

We have already alluded to the fact that during last year Mr. E. R. Waite of the Australian Museum accompanied H.M. Col. S.S. *Thetis* on a trawling and

dredging cruise under the control of Mr. F. Farnell. The cruise, or rather series of four cruises, lasted from February 18 to April 9. The coast-line covered extended from Jervis Bay to the Manning River, and, except for a trip to Lord Howe Id., the greatest distance from land was 25 miles. The depths at which the trawl was lowered ranged between 10 and 90 fathoms. The fishes were the chief object of study; about 100 species represented by 365 specimens were collected, and Mr. Waite's preliminary "Scientific Report on the Fishes" was published last year as an appendix to Mr. Farnell's "Report upon Trawling Operations." Several species are new to the colony, while a few are new to science. The entire scientific collections have been deposited in the Museum, and the results will be published as a Museum Memoir, towards the expense of which £400 was voted. On the last cruise to Lord Howe Id., heavy weather was encountered, and the passage occupied seventy hours instead of the usual thirty-six. Mr. Waite and Mr. Etheridge, who also was on this trip, were left on the island for eleven days, since the *Thetis* was blown to sea in the gale. They collected here some additional very interesting remains of *Meiolania platyceps*, the peculiar extinct chelonian, which is also found in Patagonia. Also by the help of Mrs. T. Nicholls they obtained an additional collection of shells. A large number of sponges, anemones, corals, gorgonias, echinoderms, crustaceans, and polyzoa were collected during the cruise. The number of species was very great, and included many new or hitherto unrecorded from the coast of New South Wales.

Dr. Kishinouye and other Japanese zoologists have hired a two-storeyed building on the shores of the Inland Sea, with the view of converting it into a biological station.

Professor J. Ijima has returned from a zoological expedition to Formosa.

The Danish expedition to East Greenland, under the leadership of Lieut. Amdrup, returned to Copenhagen on Sept. 13. It had investigated and mapped the tract between 65° 50′ and 57° 22′ N. lat., hitherto unvisited by Europeans. At one time it was inhabited by many Esquimaux, all of whom have now perished. A collection of their skulls and other relics was brought home. Botanical, geological, and zoological observations were made, as well as anthropological measurements on living Esquimaux in other parts. Depots were left at 60° 6′ and 67° 15′ N. lat.

Dr. Carl Peters is said to have passed from Portuguese territory into Mashonaland, after making some important discoveries of mica, saltpetre, and diamonds.

Nature reports that the Imperial Russian Geographical Society and the Ministry of Agriculture have jointly arranged for a zoological exploration of the Russian coast-line of the Pacific in the Far East. The expedition will also work in conjunction with the "Society for exploring the Amur territory," and it is intended to establish a marine zoological station at Vladivostock.

The rumour is that Nansen will not undertake another north polar expedition, but that his next trip will probably be southwards. It is also rumoured that the scientific interest of the British Antarctic Expedition is being threatened by a predominance of geographical and physical considerations. It will be deplorable if the biological problems are in any way overlooked, for the most that can be said after all is that the Antarctic fauna has been touched and scratched at.

Mr. H. J. Mackinder, the Reader of Geography at Oxford, succeeded in September in reaching the summit of the hitherto unscaled Mount Kenia in British East Africa.

Major Ronald Ross and his colleagues have been very successful at Sierra Leone, having shown that certain mosquitoes (*Anopheles* sp.) there carry the

malarial germ, and that as these breed in a few stagnant pools a little energy will suffice to get rid of them and the fever at once.

Science reports the return and the success of an expedition which sailed a year ago, under the scientific direction of R. E. Snodgrass, to the Galapagos Islands and to Cocos and Clipperton Island west of Ecuador. A large collection of animals has been made.

Prof. Georg Böhm, geologist of Freiburg, has gone on leave for a year and a half on a journey to Asia, Australia, and Central America.

An association has been formed of collectors for the purpose of exploring the local lepidopterous fauna of Hildesheim and vicinity, under the title of *Verein für Schmetterlingsfreunde.* Prof. A. Radcliffe Grote of the Roemer Museum presides.

We learn from *Science* that Profs. W. Libbey and C. M'Clure of the Peary Relief Expedition have returned to Princeton with rich collections both of vertebrates and invertebrates.

The *Scientific American* of September 23 states that a year ago Cornell University secured 30,000 acres of woodland in the Adirondack Mountains for the exclusive use of her forestry department. The land has been divided into a number of sections and several seed beds have been laid out in which there has been planted over a million small trees of different varieties. The students of forestry will study the theory of the subject from October to April, and from then until Commencement they will study the practical side of forestry. Cornell University is the only college in the United States which has a forestry department. Prof. John Gifford was recently elected to the Chair of Forestry in the University.

Nature for September 28 notes that Mr. E. R. Waite has identified the "palu" or "oil-fish" of the Central Pacific as the well-known *Ruvettus pretiosus*, hitherto known only from the North Atlantic.

The *Scientific American* reports that by a fall of rock at Niagara Falls the Horseshoe Fall has been restored to the shape from which it derives its name, which it has belied of late years.

Science reports that the German Government has sent Prof. von Volkens of Berlin to the Caroline Islands to investigate the soil and the flora.

The American Association for the Advancement of Science voted a hundred dollars to Prof. Eigenmann to help in his researches on cave animals.

The *Scientific American* refers to an interesting excursion made at the close of the meeting of the American Association for the Advancement of Science. A party went to Sandusky, Kelley Island, and Put-in Bay, at which place they explored the unique and marvellous Strontia Cave, the only one of the kind known. The arches are hung with prismatic crystals of "celestite." The place was found by Mr. Gustave Heinemann, in 1897, while opening a well. Besides exhibiting his grotto, he makes money by selling specimens of the sparkling strontia. Commercially this mineral is worth twelve dollars a ton, and is used to clarify beet-sugar, and likewise in pyrotechnics, giving a vivid crimson colour to fireworks.

At the meeting of the American Association for the Advancement of Science Dr. L. O. Howard discussed "Spider-bite Stories," and noted that he had been unable to verify a single serious or fatal case. He scoffed at the "kissing-bug" craze, which he compared to the tarantula frenzy and as in great part hysterical. He blamed the newspapers for helping to create morbid nervousness.

Dr. Howard, in his paper on "Gad-Flies" at the meeting of the American Association for the Advancement of Science, noted that before the Russian entomologist Porchinki he had tried and advocated the method of destroying these insects by means of a kerosene film spread over the pools.

Three cities contended for the distinction of entertaining the meeting of the American Association for the Advancement of Science in 1900, namely, Denver, Philadelphia, and New York. The latter was decided upon. The date was fixed for June, from the 25th to the 30th, in order to suit members who may wish to attend the Paris Exposition. The president for 1900 is Prof. R. S. Woodward, of Columbia University, distinguished for his services in astronomy, geodesy, and mathematics.

The *Scientific American* notes that the director of the U.S. Geological Survey has just issued a pamplet entitled "Maps and Descriptions of Routes of Explorations in Alaska in 1898, with General Information concerning the Territory." There are ten maps, and special reports on various expeditions, general information concerning the Territory, and tabulated information, including the gold production of Alaska. The various routes and means of transportation are clearly shown. The publication is intended for widespread distribution, and copies can be obtained by the aid of Congressmen.

We learn from *Science* that in addition to $300,000 subscribed from various sources for the endowment of Brown University, on condition that $2,000,000 be collected, Mr. John D. Rockfeller, already famous for his munificence, has offered quarter of a million dollars on condition that a million be raised before the commencement of next year.

The *Scientific American* notes that the New York Zoological Society has secured from express companies a concession in rates on live animals. Formerly the cost of transporting live animals was very high, and the reduction will be a great boon to zoological gardens and the like throughout the States.

The renowned botanist and philologist, Stephan Ladislaus Endlicher, who died in 1849, was buried along with his wife Cecilia in the Matzlemsdorfer Cemetery in Vienna. On the 21st of June 1899 the bodies were removed to a worthier resting-place near the main entrance to the central Friedhof. The Rector of the University, Prof. J. Wiesner, and the Director of the Botanical Gardens, delivered short orations in praise of Endlicher's genius and the services which he rendered to botany, philology, and science in general. (See *Verh. Zool. Bot. Ges. Wien.* xlix. 1899, pp. 359-361.)

Natural Science

A Monthly Review of Scientific Progress

DECEMBER 1899

NOTES AND COMMENTS.

Eliminated.

IT is one of the conditions of continued vigorous activity on an organism's part that income be at least equal to expenditure, and the same is true of journals. To try to sustain the activity when the aforesaid condition is not fulfilled is not uninteresting, but there are limits to the possibility of continuing it. We regret to say that we have reached these limits as regards *Natural Science*, of which this is the last number, so far as we are concerned. In spite of generous support from many during the past year, and our own endeavours in publishing and editing, the journal has not reached that measure of success which would seem to us to warrant another year's experiment. We make our bow, then, to the process of natural elimination.

Nature Studies.

THERE has been much talk of late concerning nature-studies and their more forcible introduction as part of school-education. On the one hand we hear the conservatism of those who think that education had much better continue "on the old lines," that is, without any regulated instruction regarding our natural environment except in so far as that means man and his many inventions. The proper study of mankind, they say, is man, forgetting that he does not live *in vacuo*, and is really unintelligible apart from his non-human environment. On the other hand we hear the enthusiasm of those who think that there is a new panacea for the ills of minds and morals in a codified system of scientific teaching. To any one who is acquainted with the rudiments of the rapidly advancing art of paedagogics or possessed of unbiassed common-sense, the two extreme positions seem absurd, the practical problem being to work our way towards a teaching of the humanities which will be scientific, and a learning of science which will be humanitarian.

Those who are seriously interested in the question would find food for reflection if they would take opportunity to become acquainted with "Nature Studies in Berkshire," by John Coleman Adams (New York and London: G. P. Putnam's Sons, pp. 225, 1899). It is not that there is any new discovery in the book; it is the rediscovery of delight. It will probably not even instruct, but it may possibly enlighten. It is not an educational compendium; it is a work of art. In a beautifully bound and printed volume, with fine photogravures, and in a style which sometimes reminds one of Burroughs, the author tells us of the American Berkshire; and the titles of some of the chapters will suggest his happy mood: A Whisper from the Pines, The Seamy Side of Summer, At the Sign of the Beautiful Star, The Great Cloud Drive, The Fruitage of Beauty. He excels himself perhaps in "The Circumvention of Greylock," which means "a bicycle run round a hill," but the difference between his title and ours is the difference between light and darkness. We have referred to the book here because of our conviction that its value lies in its being an expression of delight in nature by a cultured gentleman, and that if "nature study" does not at least lead towards this, it is not likely to mean more than another millstone about the neck of youth.

The Production of Parthenogenesis in a Sea-Urchin.

It is not long since Delage made a remarkable experiment, which seemed to prove that the nucleus and centrosome of the ovum were not essential to reproduction. Now comes Professor Loeb of Chicago, and, likewise by actual experiment, makes out that even the spermatozoon is not necessary. His results are given in a short note "On the nature of the process of fertilisation and the artificial production of normal larvae (plutei) from the unfertilised eggs of the sea-urchin" (*Amer. Journ. Physiol.* vol. iii. pp. 135-138, Oct. 1899). As the outcome of a long series of experiments and inductions, he was led to believe that the only reason why the eggs of marine animals did not develop parthenogenetically was that something in the constitution of sea-water prevented it. That something, he inferred from experiments on the contraction of muscles, was the presence or absence of ions of sodium, calcium, potassium, and magnesium. The two former require to be reduced, the two latter to be increased: "a great number of variations in this sense might bring about the desired effect." Without going into details, Professor Loeb states briefly that "the mixture of about 50 per cent $\frac{10}{8}n$ $MgCl_2$ with about 50 per cent of

sea-water[1] was able to bring about the same effect as the entrance of a spermatozoon. The unfertilised eggs [of the sea-urchin *Arbacia*] were left in such a solution for about two hours. When brought back into normal sea-water they began to segment and form blastulae, gastrulae, and plutei, which were normal in every respect. The only difference was that fewer eggs developed, and that their development was slower than in the case of the normal development of fertilised eggs. With each experiment a series of control experiments was made to guard against the possible presence of spermatozoa in the sea-water." Professor Loeb's conclusion is "that the unfertilised egg of the sea-urchin contains all the essential elements for the production of a perfect pluteus." "All the spermatozoon *needs* to carry into the egg for the process of fertilisation are ions to supplement the lack of" favourable ions, "or to counteract the effects of the other class of ions in the sea-water, or both. The spermatozoon *may*, however, carry in addition a number of enzymes or other material. The ions and not the nucleins in the spermatozoon are essential to the process of fertilisation." Professor Loeb believes that the same principles hold good for the fertilisation of other, if not all, marine animals, although the ions involved will probably differ in various species. By marine animals he seems to mean those whose eggs are deposited before fertilisation. At all events he does not include mammals, in which class he considers it possible that parthenogenesis is prevented only by the ions of the blood, and that a transitory change in those might allow of it.

The experiments and conclusions of Loeb are consistent with those of Delage, Ziegler, Norman, Driesch, and others. All the ideas as to the extreme importance of nucleus, and centrosome, and polar bodies and the like, are being much shaken, and it seems as if the ground were being cleared for an entirely new and far less complicated theory of sexual reproduction and heredity. It would be interesting to combine the experiments of Delage and Loeb, and to see if an ovum could be made to develop without either its own nucleins or those of the spermatozoon.

The Record of a Great Work.

In four thick volumes the famous chemist Berthelot has told the story of his work at the "Station de Chimie végétale de Meudon" from 1883 to 1899 ("Chimie végétale et agricole." Paris: Masson et Cie. 1899). The first volume deals with the experiments bearing upon the fixation of nitrogen by micro-organisms in the soil or associated with the roots of Leguminosae, by silent electrical discharges in the air, and by other means. In the second volume the central subject is the

[1] These numbers are according to corrections made by the author in a reprint kindly sent by him.

chemical history of an annual plant from germination to death, in connection with which the author recognises the enthusiastic work of his colleague, Mr. G. André. The third volume consists of special researches on the chemistry of plants, the distribution of particular elements, the alleged formation and distribution of nitrates in plants, the formation of oxalic acid and carbonates, the process of respiration, and so on. The fourth volume has mainly to do with the soil, the chemical nature of humus, and the physiological value of the various mineral substances. It concludes with an account of the author's numerous researches on the chemistry of wine. Many of the illustrious chemist's results are familiar through previous publication, and have been met with no small amount of criticism; it is all the more important that we should now have them in collected form and in detailed expression, which enables us to see more clearly the unequal strength of the evidence on which the several conclusions rest. As the record of a great work persistently prosecuted for many years and justified by many results of practical and theoretical importance, the book must command the admiration and respect of all.

Floreat Wood's Holl.

EVERY biologist who is still young enough to be enthusiastic, looks with eagerness about this time of year for the arrival of the volume of "Biological Lectures" from the Marine Biological Laboratory, Wood's Holl, Mass. The volume for 1898 (Boston: Ginn and Co., 1899, pp. 343) has just arrived, in good time for the Christmas holidays, when one can enjoy its stimuli with a less preoccupied mind. One cannot help feeling that the intellectual atmosphere of Wood's Holl must be bracing, the lectures are so vigorous.

The volume begins with a lecture by Professor E. B. Wilson on the structure of protoplasm, which we have already noticed. "The evidence indicates that alveolar, granular, fibrillar, and reticular structures are all of secondary origin and importance, and that the ultimate background of protoplasmic activity is the sensibly homogeneous matrix or continuous substance in which those structures appear." Wilson is also the author of the second lecture on cell-lineage and ancestral reminiscence—a strong plea for the acceptance of cell-homology. The third lecture on "adaptation in cleavage" is by Frank R. Lillie, who seeks to show that the special features of the cleavage in each species are as definitely adapted to the needs of the future larva as the latter is to the actual conditions of its environment. Professor E. G. Conklin discusses in the fourth lecture protoplasmic movement as a factor in differentiation, showing how delusive it is to consider the cell as if it were merely static, since movements of the cytoplasm play a very im-

portant part in developmental processes. In the fifth lecture Mr. A. L. Treadwell discusses equal and unequal cleavage in Annelids, in regard to which he seeks to show that equality of cleavage is not an indication of lack of differentiation in the ovum, for definite cells appear at definite places and at definite times, just as accurately as in unequal cleavage. The sixth lecture, by A. D. Mead, is more technical, dealing with the debatable question of the origin and homology of the prototroch. In the seventh lecture Miss Cornelia M. Clapp discusses the relation of the axis of the embryo to the first cleavage plane, and reaches "the only reasonable conclusion" that while the first cleavage plane may coincide with the median axis of the embryo, as Roux and others have shown, it is not a constant rule in any single case, much less a universal law. Dr. Thomas H. Montgomery, jun., recounts his observations on various nucleolar structures of the cell, and shows at least that both false and true nucleoli are structures of manifold complexity, in regard to which our knowledge is very vague. Dr. Watase follows with a lecture on protoplasmic contractility and phosphorescence, in which he gently leads up to the conclusion that the true physical basis of phosphorescence finds its closest analogue in the common phenomena of heat-production, and is as extensive as life itself. Professor T. H. Morgan discusses in the tenth lecture some problems of regeneration, showing that it is not easy to solve them all by quoting Lessona's law, or repeating the words "natural selection." In the eleventh lecture Professor Bumpus, who has previously made good use of sparrows, shows that they are subject to discriminate elimination. The twelfth lecture by Professor Jacques Loeb, on "The Heredity of the Marking in Fish Embryos," has been noticed separately.

The late Mr. W. W. Norman, whose loss to science is deplored, was the author of the thirteenth lecture, which shows that reactions of lower animals upon injury furnish no safe evidence of pain-sensations. Professor W. B. Scott discusses North American ruminant-like mammals in his accustomed style, and then follows a fine essay by Professor W. M. Wheeler on Wolff and the Theoria Generationis. But, in some ways, the most impressive lecture is the last, in which Professor Whitman discusses animal behaviour, and furnishes a notable contribution to comparative psychology.

The charm of these lectures may be partly due to the circumstances of their delivery, but it is doubtless mainly due to the fact that each is an expression of personal work and personal interest. One cannot but be grateful to the Laboratory at Wood's Holl, which has been the stimulus of the fine series to which this volume is added.—Floreat Wood's Holl.

Asterionella.

THE organism to which this elegant name pertains is a diatom recently investigated by Messrs. G. C. Whipple and D. D. Jackson (*Journal of the New England Water Works Association*, vol. xiv. No. 1). It causes trouble in water-supplies by producing objectionable tastes and odours. It is common in Massachusetts waters, and its recent occurrence in the Brooklyn supply led to the investigations here recorded.

The shape of this diatom resembles that of a humerus, and several cells unite to form star-like clusters. The only species is *A. formosa* (Hassall), but many varieties have been observed. The article itself must be consulted for details of structure.

The authors suppose that they saw spores or spore-like bodies in the cells, but they did not observe any of these spores (?) developing.

The diatom is widely scattered over Europe and North America, and is found in large ponds, lakes, and reservoirs, where comparatively clear water is stored.

It is said to be more abundant near the surface than in the depths. Normally it occurs in the spring and autumn, that is, regularly after periods of stagnation; but it is in ground waters stored in open reservoirs that it attains its greatest development. By ground water is meant water which has percolated through the ground.

The numbers of *Asterionella* vary from 1000 to 6000 per cubic centimetre of water. The odour at first is aromatic, then it resembles that of geraniums, and finally it becomes very fishy. The smell varies with the number of organisms in the water, and is due to a substance analogous to the essential oils.

A chemical analysis was made, and the mineral matter found to be 57 per cent of the dry weight of the organism, and of this nearly 50 per cent is silica, which is present to a greater extent in ground than in surface waters, hence the greater prevalence of *Asterionella* in the former.

The only practical suggestion possible is that reservoirs may be so designed as to be easily isolated and cleaned whenever necessary.

New Mice from St. Kilda.

MR. BARRETT HAMILTON has recently described [*Proc. Zool. Soc.* 1899] two new species of mice from St. Kilda, and his paper is of interest in its bearing on the *rôle* of isolation as a factor in evolution. At the same time, since mice are very common animals whose variation-statistics could be readily procured, one cannot at this time of day accept these two alleged new British species as securely based unless they are very thoroughly compared with the variations of *Mus sylvaticus* and *Mus musculus*. Let us illustrate our difficulties.

Alleged New Mice.

The length of the head and body of the largest St. Kilda specimen of *Mus hirtensis* n. sp. is 107 mm. for the male and 110 mm. for the female. This is exactly the size of a full-grown *Mus sylvaticus* in Elginshire. The skull of an Elginshire specimen just measured (apparently not an old one if we judge from the teeth) is 28 mm. long, 1 mm. less than the largest St. Kilda skull. The differences in ears and tail do not impress us, and still less those of colour. Even in one county *Mus sylvaticus* shows considerable diversity of coloration. At this season, when they sometimes come indoors to supplant the house-mouse for a time, specimens are trapped without a speck of yellow or brown on the side of neck or belly, while others are of a nearly uniform reddish colour on their upper parts with a very distinct line of demarcation between the white belly and sides. These are minor differences, giving no evidence of more than "individual variation." It may be, indeed, that they are merely "individual modifications" *sensu stricto.*

In Texan cornfields *Mus musculus* sometimes assumes in summer the reddish colour of some native species of Muridae, while the belly often becomes white or nearly white; the same species caught in Elginshire in October sometimes has the belly almost of the same colour as the back. It seems impossible to regard these as even varieties.

To illustrate further. A collection was made of an American species of *Cricetus* (*Hesperomys*), and the individuals were kept in captivity for a year or two. They varied in size, but did not vary much in colour, which was predominantly brownish grey. A fresh capture, however, was reddish, and suggested for the moment—we are all open to the temptation—a new variety. After some months of captivity it changed to the normal colour of the species. In all probability the original difference was simply the result of "modification."

It would be interesting to trap in Sutherland and Skye to see whether individuals of the *Mus hirtensis* type are not to be found there, for it is possible that the alleged new species is not the outcome of prolonged isolation, but was imported in hay or straw for the minister's horse a century ago.

The other form *Mus muralis* n. sp. is interesting on account of its colour, but as to its skull characters it appears to us that they will be found in perhaps every tenth *old* specimen of *Mus musculus* that comes to hand.

Our point, however, is apart from these details. It is that when we are dealing with forms for whose characters it would be easy to formulate variation-curves, this should not be neglected by those who would substantiate their claim to add new species to the British fauna.

British Mammals.

THE mammals in Britain are so few compared with other components of our fauna, that one naturally expects great accuracy in the descriptions which experts furnish. There may be better things than great accuracy, but it is at least a preliminary essential, and it is by no means always realised even in regard to British mammals. Which is disappointing.

Without ourselves claiming any infallibility we may illustrate our disappointment—made keener by our gratitude—by referring to a well-known handbook which seems to us to require a second edition. The author says that the common squirrel has a head and body about $8\frac{1}{3}$ inches in length, but every squirrel-catcher knows that a full-grown squirrel has a head and body about 10 inches in length. The picture given of the common (?) squirrel shows an animal with a tail longer than the head and body!

Of *Mus sylvaticus* the author says that it has a head and body about $4\frac{1}{6}$ in. long; the fraction suggests great accuracy, but a full-grown specimen in Elginshire often has a head and body $4\frac{1}{3}$ in. long. Of *Mus flavicollis* it is said "head and body $4\frac{1}{2}$ in. long," while of the field vole it is stated "length of head and body about $3\frac{3}{4}$ to $4\frac{3}{4}$ in. long," which surely suggests that field voles vary greatly in size, while field mice do not. Which is not the case. It is possible that the alleged species *Mus flavicollis* may be distinct from smaller varieties of the wood-mouse found in England, but in Scotland there are abundant intermediate forms, some of them as "large and handsome" as *Mus flavicollis.*

We may be making some mistake, but we are puzzled elsewhere, as when the author says "with the exception of the mouse-coloured bat, the Noctule is the largest of the British members of the order," and gives the length of its head and body as about 3 inches. But he states the length of the head and body of the mouse-coloured bat at $2\frac{1}{2}$ inches.

The author gives twenty-six pairs of teeth as the maximum in the common porpoise, but a male's skull in our possession has thirty pairs in the upper jaw. Of Sowerby's whale the author says "general colour white above and black beneath," but he must have seen the beast belly uppermost, for, when white is present, it is beneath, not above. The adults of both sexes which we have seen in the flesh had no white whatever, not even "white vermicular streaks." It is remarkable that one very distinct species of Cetacean is left out of the handbook altogether, though, judging from the number of skulls in collections, it is not the rarest one. It is needless to say that we make these remarks in no cavilling spirit, but merely to show that even in the works of experts the standard of accuracy is still not quite high enough.

Phylogeny of the Rodents.

The two preceding notes may be said to have dealt in great part with little details about Rodents, and it is at once relevant and pleasant to direct attention to a recent work which deals with Rodents as a whole. We refer to Tycho Tullberg's great work, "Ueber das System der Nagethiere. Eine phylogenetische Studie." (K. Gesellschaft der Wissenschaften zu Upsala, 1899, pp. 514, 57 plates.) Beginning with an introduction which discusses the canons of phylogenetic inquiry and the general problem on hand, the author passes to a detailed statement of his anatomical results. On the foundation furnished by these he rears his phylogenetic system, proceeding in an orderly way which it is a pleasure to follow, discussing adaptation after adaptation, and the possible causes of various lines of structural change characteristic of the sub-orders and families. The fourth part of the big book deals with the distribution of Rodents in the past and present. He attaches little importance to the alleged affinities between Rodents and Marsupials; he emphasises the contrasts between Duplicidentata and Simplicidentata, but does not think that these are inconsistent with the view that both arose from a common pre-Rodent stock; and finally he suggests a genealogical tree of the order. To discuss his decisions on affinities in brief compass would be impossible, but the work is impressive as a phylogenetic study in which a vigorous attempt has been made not only to trace the possible steps in the evolution of an order, but to detect the possible causes which determined the direction of these steps.

Phylogeny of Rust.

The origin of the rust fungi has recently given rise to a considerable amount of discussion, and Professor Dietel, in an interesting paper (*Bot. Centralbl.* lxxix. Nos. 3-4), considers the question of their descent from one or more plurivorous forms—forms, that is, which inhabited indifferently hosts belonging to the most widely different families of flowering plants. At the present day, however, only one species, a *Cronartium*, is known to retain this peculiarity, having been shown by Fischer to be capable of life on plants belonging to both Ranunculaceae and Asclepiadeae. But Professor Dietel adduces a mass of collateral evidence which seems to show that the balance of probability at least lies on the side of his hypothesis. It would indeed be difficult to account on any other grounds for the close morphological resemblances existing between forms which, while biologically distinct and inhabiting plants belonging to the most widely different families, are at the same time almost indistinguishable by any

other features. *Triphragium clavellosum,* for example, is confined to *Aralia nudicaulis,* and differs from *T. Credelae,* which lives on *Credela chinensis,* merely by the size of the spores, a difference which does not exceed the dimensions of a single micron.

Such forms must obviously be looked upon as having sprung from a common ancestor, which in this case must have lived on both hosts indifferently, especially as the two species agree in the possession of characters which distinguish them sharply from all other Triphragia.

Another example is supplied by Leptopuccinias like *P. Arechavaletae* living on Sapindaceae, *P. heterospora* on Malvaceae, *P. Elytrariae* on Acanthaceae, and *P. Lantoneae* on Verbenaceae, all of which closely resemble each other in the form of their spores and spore-beds ; while all possess in common such distinctive characters as the preponderance of unicellular teleutospores, isolated individuals of which may reach a much greater size than their fellows, and the occasional occurrence of isolated bicellular spores which also vary in size, and the septum of which is often oblique, while the only morphological differences are to be found in slight diversities in the size of the spores and in the thickness of their walls.

Further evidence of the same kind is furnished by the only three Puccinosiras known, and may probably be found in a number of other heteroecious forms.

A striking morphological resemblance is also observable between certain Leptopuccinias and the teleutospores of heteroecious species parasitic on widely different plants, but possessing aecidia which live on the same hosts as the Leptopuccinias in question, *e.g. Puccinia aecidii leucanthemi,* which forms aecidia on *Chrysanthemum leucanthemum,* gives rise on *Carex montana* to teleutospores which closely resemble those borne by the Lepto-form *Puccinia leucanthemi* on the former host.

Professor Dietel cites a large number of such correspondences, and believes that they point to the origin of the heteroecious and Lepto-forms in a common ancestor inhabiting such widely different hosts as Carices and Composites, while, on the other hand, Professor Magnus is of opinion that the resemblance is purely accidental, and ascribable to the great similarity existing among Leptopuccinias as a whole, owing to adaptation to their peculiar mode of life.

The coronate Puccinias, including, along with those heteroecious species which form their aecidia on *Rhamnus,* the two Leptopuccinias also living on the same host, and *P. Festucae,* which forms its aecidia on *Lonicera,* are distinguished from all other Uredines by the possession on the teleutospores of a crown of processes which appear to be devoid of adaptational significance, and must be considered as pointing to a common ancestry for these forms, especially as the only other Uredine inhabiting *Lonicera* is *Puccinia longirostris,* in which the crown is replaced by a single long process on the apex of the teleutospore, but

which resembles in all other particulars one of the Lepto-forms inhabiting *Rhamnus*. Fischer prefers the view that in this case the ancestral form was capable of completing the whole cycle of its life-history, as well on grasses as on various species of *Rhamnus*, and that its descendants became specialised so as to form either aecidia on *Rhamnus* and the uredo-teleutospore generation on grasses, or the aecidia was dropped and the uredo-teleutospore generation alone persisted on *Rhamnus* as in the Leptopuccinias in question.

As, however, these give rise to several generations on the same host in the course of each year, Dietel is unable to recognise any sufficient cause for the disappearance of the aecidial generation, and believes a more probable view to be that the ancestral form only bore teleutospores, and that the uredo and aecidial generations originated at a later phylogenetic stage, a hypothesis which receives some support from Brefeld's well-known views regarding the origin of the Uredines from the Auricularias, a saprophytic group which possesses no spore form comparable with either aecidio- or uredo-spores, both of which may have originated as an adaptation to a parasitic mode of existence, though not necessarily on all the host plants inhabited by the parent form.

Ferments in Fungi.

The fat-splitting ferment first obtained in a pure state by Professor Green during his classical researches on the germination of castor oil seeds, or at least a ferment possessing similar properties, has just been obtained by Mr. R. H. Biffin (*Annals of Botany*, 1899, p. 363), from a fungus which he was fortunate enough to find growing on the endosperm of a germinating cocoa-nut, and which apparently belongs to the Hypocreaceae, though to which section of the family it must ultimately be referred remains undecided, owing to the constant sterility of the perithecia, in which no ascospores have as yet been found, though chlamydospores and sickle-like microconidia are abundant on the mycelium. The fungus grows freely on sterilised slices of cocoa-nut and Brazil-nut endosperm, as well as in cocoa-nut milk and similar media, with the result that the oil which these contain gradually disappears, being decomposed into glycerine and fatty acids, the former of which is absorbed by the plant and forms its source of carbohydrate food material, while the latter accumulates in the fluid and increases its acidity.

Mr. Biffin has succeeded in isolating the ferment by the usual process of extraction with water and precipitation by means of alcohol, when a white substance was obtained, which, when re-dissolved in water, furnished a solution possessing the properties of the fungus, in so far as these are concerned in the decomposition of fats.

The same fungus appears also to secrete a cellulose-dissolving ferment, as its hyphae may be seen to penetrate with ease the walls of the endosperm cells.

More Sports.

The *Annals of Botany* (Sept. 1899) contains an interesting paper by Professor H. de Vries on the inheritance of sports. He has obtained a race of *Dipsacus sylvestris* in which the leaves are all spirally arranged, instead of being in the opposite-decussate system, typical of this plant in general. The original parents were two individuals raised from seed sown in 1884; these were carefully isolated, and from their seed 1650 plants were obtained in 1886, *Dipsacus sylvestris* being a biennial plant; but of this large number only two retained the spiral phyllotaxis. These were allowed to seed while all the others were destroyed before flowering, and the third generation, composed of about the same number of plants, contained sixty-seven twisted individuals, or about four per cent. The fourth generation gave ten per cent, but, owing to an accident, its seed could not be employed, so that another fourth generation was raised from the remaining seed of the third generation sown in 1891, and resulted in a yield of thirty-four per cent of twisted individuals, a percentage which has not been greatly exceeded in subsequent cultivations.

The gradual rise in the percentage of good plants is accounted for by improvements in the cultural methods, especially with regard to the amount of space put at the disposal of individuals, while the richness of the soil and the time of sowing are likewise factors of essential importance in the production of successful results; in short, the perpetuation of such useless if not harmful variations requires the presence of an environment as favourable as possible to the life of the plant.

A Pontifical Plant.

It cannot be laid to the charge of *Natural Science* that it has been prone to get excited over the creation of a new species, but our esteemed contemporary *Science* (October 20, 1899) has called our attention to one which affords us a purr of delight. The reference is to the *Daily Chronicle*, where botanists might naturally overlook it. As to the description of the new species, it is given in somewhat unconventional language, but this may be pardoned in the new departure of a London Daily. "The Pope takes great interest in an electric plant, to which he has given the name 'Officina Electrica

Vaticana Alessandro Volta' in honour of Volta. A few days ago His Holiness made an inspection of these plants, and the employées of the Vatican Gardens were presented to him by the Chief."

Neptuneopsis.

THE opinion is not infrequently expressed that it is hopeless now to expect novelties among the larger mollusca, and certainly the great majority of new species recently described have been of small dimensions. Now, however, as if to show how far from exhausted are the riches of the sea, we have a handsome gastropod with a shell over 16 cm. in length from comparatively shallow water (33 fathoms) off the Cape of Good Hope. It constitutes a new genus, to which the name *Neptuneopsis* has been given by Mr. G. B. Sowerby, and it has been placed in the family Volutidae, though it seems to have relationships also with the Buccinidae, Fusidae, and Cancellariidae. Perhaps, however, the greatest mystery regarding this new shell is that its publication (with a handsome coloured plate) has been undertaken by the Department of Agriculture of Cape Colony.

A Note on Inheritance.

"UNTIL recently," says Professor Jacques Loeb, "heredity has been treated chiefly as a problem for whose solution one single theory or one single principle was considered possible and sufficient." Various theories have been propounded, but none have been generally accepted. "They overlook the fact that heredity is a collective term for a series of heterogeneous circumstances which cannot possibly be explained by one principle." A more analytical study "has led to the conception that very different circumstances determine the various details in heredity," and the author gives the results of one of his studies prompted by this conception (*Biol. Lectures Wood's Holl* for 1898, pp. 227-234, 6 figs.).

But before we report on this, may we suggest that it would be clearer to agree that heredity is the most convenient term for *the relation between successive generations*, for then it is self-evident that there are several quite distinct problems to be faced. There is the question of the material basis of inheritance, whether in germ-cell or bud or otherwise; there is the question as to how this material basis has come to be what it is—capable of reproducing an organism more or less like the parent; there is the detailed comparison of one generation with another, and the attempt to distinguish how far the resemblances

and differences are due to real transmission of heritable qualities, and how far to similarity in the induced "modifications"; there is the analysis of the inheritance by statistical and experimental methods, the biggest result of which has been Galton's law; and there is the at present almost unassailable problem of conceiving how the heritable qualities work their way into realisation during the process of development—a problem that leads us away from the strict problem of heredity to that of "the principles of development." In short, we hardly think that the serious student of heredity has ever thought that he was facing one problem to which it might be expected some day to find one answer. In any case, we cannot admire the ingenious observer's phraseology when he says that "what we call heredity is composed of very heterogeneous constituents." He speaks of the so-called theories of Eimer as "nothing but a play on words," but might not Loeb strengthen his case by taking his own words more seriously?

The particular problem which Professor Loeb discusses is that of the tiger-like markings in the yolk sac of the embryo of the fish called *Fundulus*—a subject in regard to which he has previously published results. The origin of the coloration is as follows:—black and red chromatophores are found on the surface of the yolk sac; they gradually creep upon the blood-vessels and ensheath these, exhibiting chemotropism due especially to the oxygen of the blood, or stereotropism (another brave word), or both. "The heredity of the markings is, therefore, in this case determined by a stimulus which the blood-vessels exert upon another tissue, namely, the chromatophores. Both tissues are formed rather independently of each other, but from the fact that the chromatophores must creep upon the blood-vessels, and that the latter have a hereditary arrangement, the marking becomes hereditary too. This contradicts those theories of heredity which try to derive all the peculiarities of the animal from corresponding peculiarities of the sexual cell, for instance, Weismann's theory." But this is lamentable confusion; for no one surely has supposed that there are not analysable immediate conditions operative at every stage of development: the point of Weismann's theory is that the inherited organisation determines the particular occurrence and sequence of these conditions, and is thus the primary though not the immediate cause of the results.

Loeb gives an interesting figure of the tail of an embryo, in which the chromatophores are seen to have crept upon the median artery while the vein remains free. This suggests that the oxygen of the blood may be one of the causes that force the chromatophores to creep upon the blood-vessels. But this is not the whole reason, for wherever a vein is isolated they creep upon it too. Moreover, the back of the embryo is coloured black by pigment cells which follow the brain and the spinal cord.

The observations are interesting, but they appear to us to have to

do with the conditions of development, and not with the strict problem of heredity. Nor is the necessity for such investigations by any means a new discovery, for many years have passed since Professor His protested that "to think that heredity will build up organic beings without mechanical means is a piece of unscientific mysticism."

The Cell as a Unit of Organisation.

The view has often been expressed that the functions of a cell depend upon the mutual relations of its component parts. That is to say, there is a "cell-firm," in which the most important partners are the nucleoplasm, the cytoplasm, and the centrosomes, a firm which owes its power and its success to the mutualism of its partners. Dr. F. Schenck has recently published an interesting paper discussing this conception ("Physiologische Charakteristik der Zelle," pp. vi. + 123. Würzburg: A. Stuber (C. Kabitzch), 1899. Price 3 marks), in which he comes to the following conclusions:—

Not every cell can be called a physiological individual, such as a Protozoon is, for there are cells which are merely parts of a physiological individual. The process of vital combustion, and what directly depends on this, cannot be regarded as dependent on the co-operation of the characteristic components of the "cell-firm," and to a certain degree even assimilation is independent of the particular organisation. The latter is, however, implied in growth, regeneration, and differentiation; in these processes the components of the cell combine to form a unit of organisation. But sometimes the result cannot be explained from within the cell itself, but depends upon the physiological relations between the cell and the larger system of which it forms a part. The cell-structure of an organism is the structural expression of a functional division of labour in which the nucleus plays the more important (organising) rôle, while the cytoplasm is its medium reacting to external stimuli. Processes of division, in which the third important partner—the centrosome—has an influential rôle, have for their end the distribution of nucleoplasm and cytoplasm in such proportions that appropriate cellular functions continue. There is nothing novel or startling in these conclusions, but they are temperately expressed and illustrated in considerable detail; and we can heartily commend the publication to those particularly interested in cell-problems.

The Biological Corner of a Natural History Museum.

PROFESSOR L. CUÉNOT discusses in *La Feuille des Jeunes Naturalistes* (xxix. 1899, pp. 195-197) the possibility and utility of collections to illustrate facts and problems of general biology. He instances the cases in the entrance hall in the British Museum (Natural History), and some illustrations which he saw in the University Museum in Cambridge, but he protests, like Herrera, that what has hitherto been the exception should in the future prove the rule.

He takes the chapters in *L'Année Biologique*, and suggests that, although one must not expect too much in museum illustration of these, one may reasonably look for more than is at present offered. At Nancy he has himself tried to realise some of his ideals. Regeneration, parasitic castration and peculiarities of sex-inhibition, homochromy and other protective adaptations, variation, sexual dimorphism, convergence, and the like may be vividly illustrated without great difficulty. Even heredity he would illustrate by generations of mice, and the recapitulation-doctrine by placing young Comatulas in their stalked stage beside *Pentacrinus*. There is obviously no difficulty except that of time and money, which applies to other kinds of exhibits, and the pains of thought which inhibit many of these valuable suggestions. It is only fair to note, however, that the number of these biological exhibits is rapidly increasing both at home and abroad.

Linné's Type Specimens of Fishes.

ONE of the many excellent outcomes of Dr. Günther's presidency of the Linnean Society will be seen in his Anniversary Address for May last, just issued. In this address he deals with the fish preserved in Linné's own collection, which has been in the possession of the Society for about a century. How little they have been valued by the Society may best be gathered from the fact that Dr. Günther records, "in order to render them more secure in the future, your Council has ordered them to be transferred [from loose sheets of paper] to dust-proof glass-topped boxes." One only hopes that Dr. Günther will see that every precious Linnean specimen is placed in a glass-topped box before he leaves the presidential chair; it is difficult to understand why this was not done years ago.

The fishes owned by Linné consist of 168 skins, and came from three sources, Scandinavia, Germany (chiefly freshwater), and South Carolina. They are all preserved like plants in a herbarium, and on the sheets of paper are usually notes in Linné's handwriting, while those from South Carolina usually have a band of paper round the tail,

inscribed by Dr. Alexander Garden, who sent the specimens to Linné. Of these 168 skins about 40 are "types," and nearly all these came from Dr. Garden, and all of them are American.

Dr. Günther has given a careful description of each skin, the marks or writing upon it or upon the label or sheet of paper, and has added comments of his own on previous identifications, and other points of interest. Altogether a very excellent and valuable Presidential Address.

The Molluscan "Liver" So-Called.

To the student of the comparative physiology of the Invertebrates the word "liver" is a red rag. It has been applied to many different kinds of organs, and with its vertebrate connotation it has fitted none of them well. For a time, indeed, it seemed as if the recognised way of dealing with a puzzling organ was to "call it a liver and have done with it." But we have at least got beyond the stage of hypocrisy, if not of ignorance, and we speak of "the so-called liver." So at least do Messrs. Biedermann and Moritz in a recent study of the organ in question in Molluscs (*Pflüger's Archiv f. Physiologie*, lxxv. 1899, pp. 1-86), and it seems for the time a convenient device,—for the attempts to introduce such terms as "hepatopancreas," "poly-enzymatic gland," "mid-gut gland," "gastric gland," and the like have not been very successful. Do what we will, the "liver" is always with us, or with our students at least, and therefore it seems better to give it a slow death in the shackles of "so-called." But let us attend to the last news in regard to the function of this organ in the snail.

The so-called "liver" of snails contains three kinds of cells,—(*a*) secretory cells, whose secretion digests starch and cellulose in the stomach, (*b*) absorptive cells, and (*c*) lime cells. The two last accumulate stores of glycogen, fat, and perhaps some albuminoid substance. The lime-cells have especially to do with the storage of fat and calcium phosphate. The fresh secretion has no appreciable digestive effect on albuminoids. There is no absorption in the intestine, which is lined by ciliated and glandular epithelium; its fluid contents pass into the recesses of the so-called "liver" and back again. This appears to be the gist of the research, and it means another step out of obscurity.

Phylogenetic Senescence.

Those who know Professor R. Wiedersheim and his works will agree with us when we say that he cannot be blamed, as human anatomists often are, for undue preoccupation with the static aspects of man's body. In his essay on retrogression and in his book on the evidence

of the past in man's present structure, he impressed us with the idea that we carry about with us a museum of relics, that some of our structures are at present in a transition-stage of function-change, and that some parts are even progressing.

In a recent essay, entitled "Senescenza filogenetica" (*Rivista di Scienze Biol.* 1899, Fasc. iv. pp. 1-7), he has pointed out (1) that organs in process of phylogenetic regression, *e.g.* the tips of the lungs, the caudal end of the spinal cord, Morgagni's pouches in the larynx, and the posterior molars, have their weak spots, their *loci minoris resistentiae*, where they are peculiarly disposed to disease; (2) that organs and parts of organs in process of function-change, *e.g.* the thyroid, the thymus, the inferior nasal muscles, and perhaps the tonsils, are likewise peculiarly open to attack; and (3) that progressive parts, such as certain muscles and bones, are strong in their progressiveness, and less liable to disease than the parts in the two preceding categories. One cannot help wondering with another reviewer, Dr. W. A. Nagel, whether the last statement will hold good in regard to our brains, which we fondly hope are also on the line of progress.

There appear to be two distinct ways of interpreting this "phylogenetic senescence." On the one hand, we have to consider the immediate physiological conditions, *e.g.* of diminished blood-supply and weakened innervation, which may lessen the resisting power of a dwindling organ. On the other hand, we have, with Weismann, to go further back, and consider the possibility of a germinal struggle and selection among the stronger and weaker determinants, and supplementary to both interpretations there is the normal action of natural selection.

Studies in Plant Morphology.

SCHUMANN of Berlin has recently published through Engelmann (Leipzig) a second part (pp. 207-313) of his "Morphologische Studien." The studies are of a special and somewhat abstruse character, dealing with flower- and leaf-arrangement and including questions of development, mechanical conditions and the like. They will be read with interest by the somewhat limited number of botanists who can appreciate or follow such discussions. The first (No. III.) deals with the vexed question of the peculiar inflorescence in the Boraginaceae and Solanaceae, and is a criticism of a publication by Kolkwitz. The second (No. IV.) is an account of the branch- and floral-development in a commonly grown greenhouse plant, *Scirpus setaceus* (*Isolepis setacea*). No. V. deals with the leaf-arrangement in screw-pines, while No. VI., occupying two-thirds of the whole part, and entitled "The Shifting of Organs on Growing Shoots," is mainly a criticism of Schwendener's views on the same subject.

Trees in Winter.

By P. Q. Keegan, LL.D.

The external aspect of our forested and scattered trees in winter is very familiar, but the mysteries of their interior being at that season are wrapped in obscurity, and demand for their elucidation all the analytical acumen and manipulative skill that can be bestowed upon the subject. Up till within the last few years neither out-door naturalists nor arm-chair faddists cared very much about the secret arcanum, the slumbrous hibernating activities, or rather passivities, of the denizens of the forest while enduring the sharp rigours of the deepest winter. They seemed only to sleep, a few appeared to be absolutely dead, their sprouting germinative activity was no more, and save for the mystical entanglement of the leafless boughs and the picturesque intricacy of the bud-studded twigs, there was no basis, no attractive feature anywhere apparent to call forth physiological or artistic interest. If the life of the forest was to be studied and adequately comprehended, it must be done, as was thought, when buds had burst and leaves had shot forth and flowers had blown into full expansion, when life was everywhere quivering and tingling in the running sap and swollen root and stirring leaf. Such was the impression; but it was narrow and one-sided, it ignored the best half of the affair, it disdained the law that organised matter adapts itself to circumstances, to the wintry chill as well as to the sultry glare, that it operates by counterparts, so to speak, neither of which is complete, but each a supplementary constituent of the grand totality.

The justification for the foregoing remarks will, I think, be found by any one who cares to make himself conversant with the history of scientific research anent the winter life of our trees. Previous to the year 1870 a few plant analysts and botanical chemists had investigated various parts and organs such as barks, buds, etc., gathered during the winter season; but at all events, in 1871 Richard observed that in the month of February there was a deficiency of starch in certain twigs of willow, linden, and birch, which cases, however, he considered to be mere exceptions to the law broached by Mohl, founded by Hartig and Sachs, and generally held true at the time, viz. that the reserve starch

undergoes no change in winter. N. J. C. Muller was the first to observe the disappearance of the rind starch in winter, and he thought that it migrated into the wood. Russow, in the winter of 1880-81, examined the barks of ninety-two different tree stems up to sixty years old, and found starch only in ten kinds, but as compared with the autumnal content it showed a great diminution; experimenting again in the colder winter of 1882, he found that in all species of tree the starch had disappeared up to isolated traces, it being transformed principally into fat-oil; on the other hand, he came to the conclusion that the starch had all the time remained unchanged in the wood of all the species investigated. He found, moreover, that towards the end of March the rind starch had been copiously formed again, *i.e.* long before the bursting of the buds, the suppression of the carbohydrate thus lasting from November till April. In 1884 Baranetzky and Grebnitzky published the results of their researches. They found that not only the rind starch but also the wood starch was reduced in winter, and may even disappear altogether, *e.g.* in lime tree, fat-oil stepping into its place; on the other hand, in hard-wooded trees, while the starch vanishes entirely from the rind, it remains, though somewhat reduced in quantity, in the wood. In 1890 Dr. A. Fischer confirmed the views of these observers, and further investigated the method and course pursued in the process of the starch dissolution in fat-trees during the autumn. He emphasised the opinion that the entire wood-starch of the younger twigs in fat-trees is transformed on the spot, *i.e.* the principal mass of the starch undergoes no translocation. He was also disposed to conclude that the greater part of the fat in the older wood of certain trees is never changed at all, whereas that contained in the rind disappears almost entirely in spring and summer. He further recognised eight phases of starch transformation, viz. a maximum in October and in April, a minimum in December, January, and February, and again in the latter half of May, a dissolution in November and beginning of May, a regeneration in March, and a storing up from June till October. In 1891 Monsieur Emile Mer, who had studied the distribution of starch in the principal trees and indigenous shrubs of France, found that in the middle of November a great change had already been wrought, the starch had nearly all disappeared from the cortex and liber at least in the branches as well as in the middle and upper parts of the trunk; in the wood it had notably diminished in white-wooded trees, though still abundant in hard-wooded trees, while plants with persistent leaves hardly held it any longer save at the base of the stem and in the current year's twigs chiefly on a level with the buds. He found that the starch gradually passes from the wood into the liber, first from the medullary rays, then from the wood parenchyma, and finally from the medullary sheath and pith; the rays of the young liber are the last to yield up the vanishing starch. Apparently this absorption must needs, he

thinks, be attributed to the respiratory combustion exerted by the woody and liberian tissues from the moment when the leaves have lost their assimilatory activity up till the beginning of the winter rest. "As long," he says, "as a certain degree of moisture remains in the tissues, life is maintained there, and it may occur that the starch reserve is entirely absorbed. In the same way, after the fall of the leaf, woody plants still continue for a certain time to vegetate and to respire; it is in the liber that this function seems to be most active and most persistent. It is not only a more or less complete absorption of the starch reserve that is produced in autumn, it even works a profound change in the distribution—due to this that the foci of attraction are displaced. It is known, in fact, that starch is borne always to the points where vitality is most developed. Now in this season the only regions where there still remains a residue of vegetative activity are, on the one hand, the buds which the young branches bear, on the other the roots whose vegetation is prolonged for a certain time after that of the aerial organs exposed to the first cold. In proportion as the season advances the respiratory combustion slackens, and from the moment when the vegetable enters into the period of latent life, the distribution of the starch remains stationary during nearly three months" (*Comptes Rendus*, vol. cxii. for 1891, p. 964).

I think it would be difficult to quote or translate a passage which reflects more faithfully and lucidly the inevitable results of true scientific observation and experiment upon the veritable scientific intelligence. The facts are interpreted aright and referred back, so to speak, to their real, *i.e.* their physiological cause. It has been maintained that the winter period of rest of our deciduous trees is not dependent on external conditions, such as temperature or moisture, but on internal changes, and especially on such as enable the starch-containing cells to transform their starch into sugar. Sachs thought that possibly there must be a very slow production of ferments before the buds can develop in spring, as they cannot by any means be caused to develop in autumn or beginning of winter, although meanwhile their reserve materials are not chemically changed. Nevertheless, a study of the actual condition of affairs within the veil of mystery that enwraps the winter forest, reveals the groundlessness of the opinion that the reserve materials are chemically unchanged. In point of fact, we discern that the protoplasm contained in the delicate, colourless tissues of the bark becomes very rich in fatty matter, probably operative as a resistant to the extremes of cold. The starch, moreover, which in most of our trees is laid up in the wood and in special reservoirs below the buds, is during the hard season very rich in substance and poor in water, the grains seem to be smaller than in summer, and amylodextrine seems to accompany it, *i.e.* altogether it is hardly in a condition well fitted for chemical transformation. No doubt glucose or other combustible carbohydrate may gradually all the while be

entering into solution in the cell sap; but the quantity thereof must be extremely small, perhaps not much more than sufficient to forefend the utter and final extinction of the feeble spark of life that continues to glimmer amid the bitter cold and benumbing surroundings. Moreover, although it is recognised that ferments are the products of cells in process of disorganisation, there is some doubt whether even this process goes on in the dead waste and middle of winter. Rather a universal torpor seems to reign in the domain of plant life, and, in a general way, "as you were" is the word of command from November till March. On these grounds, therefore, and for other reasons too abstruse to be succinctly recited, I am disposed to conclude that the winter period of rest, even in our evergreens, does actually and principally (I do not say entirely) depend on the external conditions to which the plant is subjected. In the case of the Coniferae, their limitation of growth towards the north is due to dry winds on sunny days in winter stimulating transpiration at a time when the roots can draw no fresh supplies of moisture from the frost-bound soil. Hence in the evergreen leaves of this order, special protective contrivances against excessive transpiration are indispensable. In our deciduous dicotyledonous growths, on the other hand, these special defences are apparently incompatible with that full and free activity of the chlorophyllian protoplasm in summer which is necessary to build up characteristically hard woods.

Descending now to particulars, it is proper to mention that what Fischer has termed fat-trees are those which are soft-wooded, and contain at the period of the starch minimum in winter (December, January, and February) no starch at all in the rind, wood, or pith, *e.g.* Scotch fir, birch, alder, poplars, lime, *Robinia*; in spruce fir, larch, yew, juniper, etc.,the wood never becomes completely devoid of starch, but even in these fat predominates in the wood in winter. Starch-trees, on the other hand, are hard-wooded, and while in winter the starch disappears completely from their bark and pith, it remains almost unchanged in quantity in the wood and medullary sheath. The ultimate cause of these differences seems to be that the assimilatory activity of the foliar organs of the trees in the first category is not so active as it is in those of the other. More starch is produced in the leaves of the latter, the starchy reserves of the medullary rays and wood parenchyma are more redundant and not so readily exhausted; hence vitality is more developed, the annual rings are broader, and the excess of plastic substance is used up in the thickening of the autumn zone of wood, the whole contributing to raise its density and hardness considerably as compared with that of firs, pines, and other fat-trees. My own investigations lead me to consider that the wood of conifers is very poor in starch at all times, even in isolated trees developed in the highest noon of summer; while again, although at this season the wood of birch, alder, lime, etc., is very rich in starch, it, even before

the leaf falls, easily degrades and suffers what may be called a dextrine change.

Is the process of deassimilation likewise checked and brought to rest within the inner arcanum of our trees in winter? Do tannoids, resins, volatile oils, waxes, tannins, and coloured pigments continue to be produced as the outcome of the spent and exhausted energy of the chlorophyllian protoplasm? "Assimilation," says Mesnard, "may be very feeble and even be annulled completely, but deassimilation should not be null as it is indispensable to the proper functioning of the cell." Wigand, in a general way, declared that the young shoot in the condition of winter-bud contains no tannin, but has starch; he imagined that the tannin is changed into starch, and in that condition held as it were its winter sleep. In 1875 Oser concluded that the tannin of the current year's twigs of oak decreased in winter, it being possibly used up in a kind of internal respiration, the tannic acids being very easily oxidisable. In 1888 E. Schulze discussed the question, Are the leaves of evergreen trees emptied in autumn like caducous leaves, or are they filled with reserve materials like the other persistent organs? He performed numerous micro-chemical experiments, and concluded that only in Gymnosperms and in most Dicotyledons do the leaves serve as magazines of reserves during the resting period. He found that not only starch but fatty oil and tannin may still be detected in the winter foliage; sometimes tannin is found there alone, sometimes it exists along with starch or oil, but they are rarely found side by side in the same cell; moreover, when oil accompanies tannin, the cells which contain the oil are generally deprived of starch. Starch and tannin occur in the winter leaves of oak, holly, mistletoe, spindle-tree, etc., whereas those of ivy, guelder rose, firs, pines, etc., contain tannin only. G. Kraus carefully examined the youngest shoots of several trees and shrubs monthly during the winter, and found that the tannin of the twigs formed in the preceding vegetation period undergoes no change in the winter months, and hence Oser's idea of its mission as a respiratory material falls to the ground, and Schulze's and Haberland's opinion that it is a reserve substance is consequently unsatisfactory.

Nevertheless, I think there is some satisfactory evidence to prove that if tannin, *i.e.* the capital product of deassimilation, does not increase during the dead months of December, January, and February, it at all events develops to some extent; this is to say, by further exposure to the oxidising agencies of light and air it suffers dehydration, or a molecular rearrangement of its constituent atoms. For instance, it is during the wintry gloom that the leaves of ivy assume their brightest red, the buds of the Norway maple are red in autumn but become of a still darker red in the course of the winter, the holly berry never shows so ruddy a radiance as about the merry Christmas time, and many other illustrations may readily be recalled. Indeed, from the

analogy of the autumn manifestation of colorific effect investing the woodlands at a time when the assimilatory activity becomes dull and deadened, there is nothing unreasonable in the assumption that oxidising effects continue to be produced later on when only a feeble minimum of protoplasmic respiration remains as the last remnant of vitality. Even in dead leaves the glucose and other autoxidisable substances disappear, at least in part, as the result of the direct action of atmospheric oxygen. So that whether the process be regarded as either physiological or chemical or both combined, it would be absurd to imagine that a substance absorbing oxygen so readily as tannin does, can remain totally unaffected through fresh winds, sunny skies at times, and a small absolute content of aerial moisture. Judging from the analogy of the fruit, wherein tannin remains long and in some cases even is completely destroyed by oxidation, it would seem that the tannin of the winter boughs and leaves gradually becomes, as the season advances, more complex in composition, less easily crystallisable, and less soluble; possibly it takes up new carbon radicals, whereby, while retaining an analogous-chemical constitution, its reducing properties are not diminished.

It might be imagined that a property like wax-formation, suberification, etc., to which plants owe their great power of resistance to the effects of climate, would, if not specially prominent, be at all events not altogether suspended during the winter months. It appears, however, that even in these respects the palsying, life-consuming influences of cold are not arrested. "The resin and wax metamorphosis are probably conditioned by a slackening of the cellular activity," says Wigand. On the other hand, according to Uloth, who had carefully studied the wax-formation in *Acer striatum* and other trees, this process is not a physiological but rather a purely chemical one, requiring a peculiar condition of the cellulose, with the co-operation of light and of a certain high temperature, and hence takes place only during summer. "During the winter," he states, "as is seen distinctly after the fall of the leaves, the wax-forming process stands still in order to begin anew with the second spring entirely in the same way as before." This attestation is of some importance, inasmuch as it throws some light on the vexed question of the precise physiological character and position of a substance, the origin of which has proved rather a bugbear to all serious students of arboreal chemistry. My own impression is that wax, suberin, etc., represent the products of chemical decomposition (deassimilation) resulting from the specially vigorous and rapid activity of certain locally restricted non-sexual propagative cells, such, for instance, as compose the phellogen and the epidermis of young leaves; and this being so, the fact that this unwonted energy is arrested in winter becomes easily explicable, and is by no means extraordinary.

Not the least remarkable of the phenomena connected with the

winter rest of our trees is the lavish accumulation of oxalate of calcium in the buds and even in the pith of the young shoots. A section made even in October through the bud of sycamore, alder, or ash reveals an extraordinary state of affairs. What does it all mean? The cells seem reeking, as it were, with large or small crystals well and truly formed. In sycamore buds these are very large, while in those of the ash they are of all sizes apparently. A transverse section of the bud-scales of the latter tree shows a peculiar collenchymatous tissue filled with a thickly granulated plasma which invariably encloses among several tiny rodlets of oxalate of calcium, a large octahedron of the same substance. "The oxalate formed in the autumn in the buds is still unchanged in spring," says Wehmer. Kraus concludes that it is a reserve food material and not an excretion, and is taken up in spring; but after careful study and consideration I am disposed to conclude that it merely represents an oxidation product of the carbohydrates; in fact, it is the result of a specially active metabolism connected with the molecular rearrangement of certain carbohydrates while being subjected to an unwonted and extraordinary intensity of respiration.

Patterdale, Westmorland.

Lacepède's "Tableaux . . . des Mammifères et des Oiseaux," 1799.

By C. Davies Sherborn.

In *Natural Science* for December 1897 (p. 432) there will be found a letter by me upon Lacepède's "Tableaux." These "Tableaux" are classifications of mammals and birds, and their interest consists in the fact that in them many generic names are used for the first time in literature. In the communication referred to above, I mentioned that an edition of Buffon had been discovered in which these "Tableaux" occurred, and that the volume containing these "Tableaux" was dated 1799. As this was the date of the original publication, and as the original publication had been lost sight of practically since it first saw the light, the discovery was apparently of considerable importance.

In June 1899 I received a kindly communication from Mr. C. W. Richmond of the United States National Museum, pointing out that according to the *Journal Typographique* of Paris, the edition of Buffon in question, although dated 1799, apparently came out in livraisons of two volumes a month, and that the particular volume containing the "Tableaux" (vol. xiv. of the Quadrupeds) was not published until October 1802. Mr. Richmond has since published the whole story in *The Auk*, vol. xvi. No. 4, October 1899, pp. 325-329.

Now let me deal with the Buffon first. The edition in question was published in 76 vols. 18mo, Paris, 1799-1809. It was printed by Plassan, and published by Saugrain. At the conclusion of the 35th livraison (vol. xiv. "Quadrupèdes," and vol. x. "Poissons"), that is in Oct. 1802, the entire work was purchased by the brothers Didot (*Journ. Typographique*, August 25, 1803, p. 358) and reissued with new title-pages, bearing their imprint instead of that of Saugrain, but carrying the same date, viz. 1799, the date in the original issue being expressed, "L'an vii de la République," and in the Didot issue as "An vii—1799." The Saugrain title-pages were torn out and the Didot title-pages pasted upon the guard thus left. Didot then issued in 1804 vols. xi.-xiv. of Poissons, and in 1809 vols. i. ii. of Cétacés, thus completing the 76 vols. The copy in the British Museum (Nat. Hist.) is of the second issue, with the exception of vol. i. of the "Quad.

Ovipares," which still bears the Saugrain title-page; Professor Newton has the greater part of an original set containing some of the Saugrain title-pages, which formerly belonged to Mr. de Winton, while Mr. de Winton has recently acquired a second set, bearing the Didot replacements. Now it seemed quite possible that the Saugrain issue was published as a whole at first, and proving unsaleable was afterwards issued in livraisons of 2 vols. per month; but it was not so. Mr. Richmond's authority (*Journ. Typ.*) is quite accurate, for it is proved to be so by a reference to the *Journal Général de la Littérature de la France*, which not only gives from month to month, practically the same information as does the *Journ. Typ.*, but definitely states on 7 Nivôse vii. (December 28, 1798) that Plassan would publish an 18mo ed. of Buffon by Lacepède, 2 vols. on the 1st of each month, beginning Floréal vii. (Ap. 20, 1799). The *Journ. Général* also announces the final volumes, xiv. "Quads." and x. "Poissons," in Brumaire, xi. (October 1802). Probably Saugrain dated his volumes l'an vii. because they were all printed and ready to issue by that year, but why Didot so dated his new title-pages is a mystery. Engelmann, *Bibl. Hist. Nat.* 1846, p. 322, refers to a separate issue of the "Tableaux" in 1802 in 18mo, Plassan; and among the bibliological treasures of Professor Newton is a unique copy of this, containing only the birds, which belonged to Fischer de Waldheim, and has his annotations throughout. It is repaged but otherwise identical with the corresponding part of the birds in vol. xiv. of the "Quads." in the Didot Buffon.

So much for the Didot Buffon, for much of the elucidation of which we must thank Mr. Richmond. Let us now consider the history of the "Tableaux." As stated in my previous letter, Lacepède read his paper on Mammals before the Institute on 21 Prairial An vii. (June 9, 1799), and his paper on Birds 6 Fructidor An vi. (August 23, 1798), or nearly a year before. They were published together in the *Mémoires de l'Institut*, vol. iii., in 1801. They have been frequently quoted as 1799, but no copy has ever been produced in support of the statement. I imagined I had found the 1799 issue in the Buffon, but Mr. Richmond has argued that I was mistaken. There was still another reference in Engelmann (p. 376) to an issue "in-4, *Paris, An* vii. (1799), Plassan (38 pages)." This tract I had searched for for years, and almost despaired of obtaining, when quite by chance I discovered its more exact title, made a fresh attempt, and after several months succeeded in securing a partly uncut copy. Here is the full title, "Discours | d'ouverture et de clôture | du cours | d'histoire naturelle | Donné dans le Muséum national d'Histoire naturelle, | l'an vii de la République, | et | Tableaux méthodiques | des mammifères et des oiseaux, | par le C[en] Lacepède, | De l'Institut national de France [7 lines of titles, etc.] | a Paris | chez Plassan, Imprimeur-Libraire. | L'an vii de la Republique. | —These "Discours" occupy 55 pp., p. 56 is devoted to "Errata," and then follows "Tableau des divisions,

sous-divisions, ordres et genres des mammifères par de C^en^ Lacepède," [etc.] "A Paris, chez Plassan [etc.] L'an vii de la République," pages 1-18; followed by "Tableau des sous-classes, divisions, sous-divisions, ordres et genres des oiseaux" [no title-page or date], pages 1-20, and "de l'imprimerie de Plassan."

The whole is bound in the original boards, and is uncut in two places; is obviously a complete and perfect book; contains the 38 pp. tract referred to by Engelmann on p. 376; and was reviewed in *detail* in the *Journ. général Litt. France*, iii. 1800, Nivôse An viii. (December 21, 1799, to January 19, 1800), p. 7. Having, therefore, found this missing book, we are now in a position to speak definitely as to the date of Lacepède's generic and specific names. They first appeared in print quite towards the end of 1799, in the newly found tract. They were quoted by Daudin in his "Traité Orn." 1800 (Richmond, *Auk*, xvi. p. 327); they were printed in *Mém. de l'Inst.* iii. 1801, reset and repaged, and with the "errata" which appeared in p. 56 of the "Discours" corrected; they also appeared, with additions by Daudin, in vol. xiv. of the *Quadrupèdes* of Didot's Buffon in October 1802; from which volume separates were issued in the same year, of which one copy, and that of birds, is known. It is further to be noted, as a correction of my previous note, that of the *specific* names Lacepède supplied one each to the mammals, as seen in the original tract and in the *Mém. de l'Inst.*, while the whole of the other specific names are Daudin's, and their date must of course be a matter of separate individual inquiry.

Professor Newton, who has followed the whole inquiry in the kindest and closest manner, has carefully examined the 1799 tract and says: "My impression is that the two 'Tableaux' (Mammals and Birds), each of which has its own pagination, distinct from that of the 'Discours,' must have been printed *before* the 'Discours' were—and the Birds indeed so long before as An vi., on the 6 Fructidor, of which year the *table was shown* to and a memoir upon it read before the Institute, as he himself [Lacepède] stated on the 21 Prairial of the following year, when he presented the Mammal scheme. You will observe that the 'Tableau' of Birds in your copy has no separate title-page, and it looks as if that was printed off in An vi.—the 'Tableau' of Mammals not being printed till the following year—in which year also the little Buffon, according to its original title-page ('chez Saugrain,' etc.), was printed, *i.e.* 1799, while in my view the original 'Tableau' of Birds belongs to An vi., *i.e.* 1798! Whether *publication* in the strictest sense of the word can be claimed successfully for that date is more than I should like to say; but there ought, I think, to be no doubt that his genera both of Mammals and Birds were published in An vii. (1799)."

In a further letter Professor Newton points out, that while in the *Mém. de l'Inst.* the errata that appeared on p. 56 of the "Discours"

were corrected, they were *not* corrected in vol. xiv. of the "Quadrupèdes" of the Saugrain Buffon. And this fact seems to suggest that the seventy volumes issued by Saugrain were all printed in or before l'an vii. (1799). Why their publication was delayed, or arranged as two volumes a month, we may never know.

It follows, therefore, that—

Lacepède's "Tableaux," 1799, exist.

Their date is 1799.

All the generic names are Lacepède's.

The specific names in the original tract and in *Mém. de l'Inst.* are Lacepède's; but all the rest which appeared in the 1802 volume are Daudin's.

I have entered rather fully into this subject, and if the general reader of *Natural Science* considers the whole thing a bore, it will at least show him that much and tedious labour is necessary before one can solve so apparently simple a problem as the date of publication of a mere name, or even of an individual volume.

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An Extension of the Method of treating Variations, with Examples and certain Conclusions.

By H. M. Kyle, M.A., B.Sc. (St. Andrews).

It is proposed to give in this paper a short account of an extension of the method at present used in the study of variations. Examples will be shown in order to illustrate the working of this method, and a brief discussion of the conclusions towards which the results already obtained point will be added.[1]

It is unnecessary to enter into the details of the present method, since they are now so well known, and only the underlying principles will be mentioned in order to preserve the continuity of the subject and display the exact point of the new departure. For the sake of clearness the various stages will be denoted by propositions, three in number: (1) the application of the laws of probability; (2) a law which holds for all the individuals of a "group"; (3) a formula for determining to which of known groups any chosen individual belongs.

1. The variations of any organ or part of an organ in a series of individuals of the same race or species conform to the laws of probability. When arranged in order these variations form a curve which may be expressed by one of several algebraic equations. The most common of these equations is that known as the "Probability Integral." Further, when the variations of one organ have been expressed, a constant can be found showing the relation of these variations to those of another organ; in other words, the correlation of organs can be expressed mathematically.

With two great exceptions the examples hitherto given have been concerned with the variations of particular organs and the correlation of these variations. The conclusions have been restricted for the most part to displaying the "fact" of variation and the importance of the mathematical method. More recently an effort has been made to pass beyond this stage and connect the observed change in a range of variations at different times with a known change in the environmental conditions.

It is necessary here to enter into a slight criticism of this position

[1] For conclusions, see pp. 417 *et seq.*

in order to contrast it with the position shown later. If it is sought to connect directly the rate of variation in any one particular organ or part of an organ with a certain change in environmental conditions, it is not difficult to show that the conclusions reached will depend much more upon the pre-existing assumptions with regard to the relations of the organism and environment than upon the actually observed facts.

This can be shown mathematically. If $\phi(x)$ represent a group of organisms, *i.e.* their characters, let x_1 be one single character; $\phi(x_1)$ will then represent this character in the group and $\phi'(x_1)$ its rate of variation. Further, let $f(x)$ represent the group of environmental conditions, then $f'(x)$ is the rate of variation of this group. That $\phi(x)$ varies with $f(x)$ is the accepted position in biology.

That $\phi'(x_1) = kf'(x)$ is the above position with regard to the rate of variation in a single organ, where k is a constant depending on $f'(x)$. But x is composed of many variables, say x_1, x_2, x_3, etc., hence the true relation between the rate of variation in $\phi(x)$ and $f(x)$ must be $\phi'(x_1\, x_2\, x_3 \ldots) = kf'(x)$. These two equations cannot both be true except on two extreme probabilities: that the other organs do not vary, or that the rate of variation is the same for all. This means that $\phi'(x_2)$, $\phi'(x_3) \ldots$ which include the variations due to growth and correlation, are all equal and each $= \phi'(x_1)$. This assumption is obviously a very great one, but even then we have only come to the observed fact that $\phi'(x_1) = kf'(x)$. We come now to the conclusion that k measures the rate of change of $\phi(x_1)$ with regard to $f(x)$, but what then? The meaning we give to k must obviously depend upon the assumption we make as to the relations between $\phi(x)$ and $f(x)$. In other words, k cannot be taken to prove our original assumption. There seem to be but two ways of regarding the relation between the rate of variation of an organ and a change in the environment, the one that the relation is direct, the change in the environment causing the alteration of the organ during growth; the other that the relation is indirect—the change in the environment bringing about the alteration in the organ by destroying the individuals which did not possess the actually observed altered organ. In the former case, k is like any observed constant in the science of physics; in the latter, it is a measure of natural selection.

The question then comes to be, which of these assumptions will best explain the facts? Hitherto the theory of natural selection has flourished under the belief that it could explain the facts rather than that the facts were rightly explained. In the conclusions of this paper an endeavour will be made to show how this theory rests on an assumption which, however probable in appearance, must always remain unproven, and it will be suggested that the counter-theory explains the facts better.

If it is difficult to make a just comparison of the changes in a single organ with the changes in the environment, it is equally difficult, on the other hand, to make such a comparison for the species. It is the "species" that has formed the starting-point of the theory of natural selection, and by the light of the "species" the structures of the individual, its birth, every portion of its life, and even its death, have been interpreted. But the "species" is a quantity not easy to measure, and it thus seems very wide of the mark to talk of a

character, or the variation in a character, as being "good for the species," as having a "selective value," when nothing definite is known.

On the one side, therefore, we have "organs," on the other side "species"; and when we consider things as they naturally are, over all is the environment. Is it not possible, then, to find a method of grasping the mean? Between the organs and the species lies the most real of all, the individual—the unity of biology. If we could but understand the single life in its entirety through concentrating and testing on it all the conceptions of biology, we should know better the meaning of "change"—how it arises, and thence also the meaning of "evolution."

This is the background of the task which Professor Heincke[1] set himself. After various trials at combining variations, and the making of formulas to represent groups and species, he advanced towards a method of determining, and a conception of the individual—not as an abstraction, but as something real and composed of organs, and forming one of many exactly equal under equal conditions.

The method consists, not in the correlation of the variations of two or three characters, but in the correlation of the averages of as many characters as possible. If the variations of many characters are obtained, those of each character may be arranged about the centre 0, as in the ordinary mode of dealing with variations. Hence a system of groups of variations is obtained, each group representing the most probable distribution of the variations for that character, and the common centre representing the average of each group. If the variations are then arranged in parallel columns they may be summed up and treated as if they were deviations from the common average at 0.

In other words, the sum represents the distribution of the variations just as the ordinary arrangement of deviations about an average represents their chance distribution.

The assumption underlying this method[2] is that each group of variations conforms to the same "type" of probability, that of the "probability integral." This assumption has already been challenged as if it invalidated the whole principle, but although it is not easy to say what correction should be made, a summation is certainly possible, and the warrant of its being near the truth is shown in the results.

The law which arises from this summation holds good over all the individuals of a group under the same conditions. It gives the second proposition.

2. The standard deviation of all the variations of the individuals when grouped about the common centre 0 is 1·18, and the probable

[1] "Naturgeschichte des Herings," *Abhandl. des Deutsch. Seefisch.-Vereins*, B. ii. H. 1, 1898.

[2] A further assumption is that each character presents an independent series of variations.

error 1. The proof of this proposition here given is slightly altered from that given in the original, but seems a little clearer.

Let k be the number of characters,
n the total number of variations,
d any deviation of any character,
v the corresponding variation,
w the corresponding probable error,
and N the number of variations of any character.

Then
$$\frac{\text{Sum of all the variations}}{\text{Number of variations}} = \frac{1}{k}\Sigma\left(\frac{\text{Variations of any character}}{\text{Number of variations of this character}}\right)$$

$$i.e.\quad \frac{\Sigma v}{n} = \frac{1}{k}\Sigma\left(\frac{v_1 + v_2 + v_3\ .\ .\ .}{N}\right)$$

$$= \frac{1}{k}\Sigma\left(\frac{\dfrac{d_1 + d_2 + d_3\ .\ .\ .}{w}}{N}\right)$$

$$= \frac{1}{k}\Sigma\left(\frac{\dfrac{d_1 + d_2 + d_3\ .\ .\ .}{N}}{w}\right)$$

$$= \frac{1}{k}\Sigma\left(\frac{\text{Average deviation}}{\text{Probable error}}\right)$$

but on the assumption that has been made, if the number of individuals and characters observed be increased, each fraction on the right-hand side of this equation tends to an equality, and from the probability integral—

$$\frac{\text{Average deviation}^{1}}{\text{Probable error}} = \frac{1}{\cdot 8453} = 1\cdot 18.$$

Hence, the sum of all the variations [1] divided by the number of variations is equal to 1·18, and this is the standard deviation of a curve whose centre is at 0 and whose probable error is 1.

Two examples may be given in order to illustrate this conclusion. The first is of a group of 50 herring from the White Sea, and is taken from the work of Prof. Heincke.

$\frac{\Sigma v}{n}$ for 5 characters,	differs from 1·18		by 0·40
for these + 3 more,	,,	,,	by 0·29
for 13 other characters ,,		,,	by 0·25
for all (21) ,,	,,	,,	by 0·21.

The difference between theory and observation is evidently very small. In the second example the variations of 54 plaice from St. Andrews Bay are tabulated in full according to the method. Under the second

[1] The terminology used here is that of Galton, Weldon and Pearson. "Deviations" are the observed fluctuations about the average of any character, "Variations" these deviations when expressed in terms of the probable error. These correspond with the terms used by Heincke.

heading are arranged the frequencies of the variations, under the third the average or standard deviation of the variations for each successive group of five characters, and under the fourth the probable error of each group.

Character.	Deviations[1] expressed in terms of the "Errors of Mean Squares."													Average Deviation of Variations.	Probable Error.
	−3	2·5	2	1·5	1	·5	−0+	·5	1	1·5	2	2·5	3+		
Intermaxilla (l)		2		9		20		14		7		2			
Intermaxilla (a)	3		5		14			24		4		4			
Tail (l)		1		10		15		14		9		5		1·14	·96
Eye (l)	1	1		6	12				8	6		5			
Mandible	1		4	6	4	4		9		5	2	2			
Head (d)	3		5		10				6		9		4		
Head (b)	2	3	0	7	0	11		18	0	7	0	5	1		
Head (l)		6			16			21		7		4		1·42	1·29
Body-Height	2	0	4	5	7	8		4	5	0	8	5			
C. Vertebrae	4												7		
A. Vertebrae	5												2		
Fin-rays P(r)	2				24				26				2		
" P(l)		1		8					13			2		1·33	1·12
" A	1	4	0	4	8	6		4	6	5	1	2	3		
" D	1	2	3	7	3	12		8	6	0	3	1	3		

Average deviation for all the characters 1·29
Probable error " " " 1·09.

The conclusion one would draw from these two examples is that the assumption which permits theory and observation to agree so closely cannot be far from correct. The characters were not taken in any fixed order, and the results show how the observed values fluctuate about the theoretical, the first group having an average deviation lower than the theoretical value, whilst the second and third have higher. This fluctuation gives point to an important corollary founded by Prof. Heincke on the theory shown above, namely, that if one or two characters only of all the individuals of a fixed group could be examined the variations would also agree with this law. The difficulty of obtaining this fixed group of individuals similar in all respects as to age or size, place or living conditions, etc., is therefore the only drawback.

A second corollary of even greater importance is also deduced from this theory. If this formula represents the "essence" of the variations of all the individuals, the conditions being the same, it must also do so for one. The variations of each individual are equally balanced on either side of the average, that is, if the variations of all the characters of each single individual could be tabulated they would be distributed about the mean according to the above law.

[1] The signs + and − are not used in obtaining the "Average deviation."

From this theoretical conclusion we are led on to the practical outcome. If the conditions are altered we get a second group of individuals each of which conforms to the same law, but the centre of the averages is altered. Hence if any individual be chosen at random from one of these groups we should be able to tell by an examination of its characters to which group it belonged. Hence the following practical rule.

3. The sum of the squares of the variations in the characters of a certain group is a minimum for the individuals of that group.

This follows directly from the equation of the probability integral,

$$y=\frac{1}{k\sqrt{\pi}}e^{-k^2x^2}$$

the nearer y approaches the centre of the curve, or the average, the smaller x^2 becomes. This being true for all the characters, we have that Σx^2 is a minimum for the variations in the characters of a certain group. Hence, if it is desired to know to which of several known groups a certain individual belongs, it is necessary to calculate the variations of each character of the individual from the respective averages of the several groups, then find the sum of the squares of these variations, and the least sum shows the group to which the individual is most nearly allied. The more characters that are taken the more likely is the result to be right, but less characters are necessary the greater the number of individuals. The first example taken to illustrate this is one of several given by Prof. Heincke. It refers to a single specimen of the group of herring obtained from the White Sea, which had 58 vertebrae where the average was 53·6. One might think, therefore, that this individual was abnormal for this group, or belonged to quite another group. Two other groups are therefore taken, the one from the west and south-west coast of Norway (Vaarsild), which has 57·5 as the average number of vertebrae, the other from the Jutland Bank off Denmark, which has 56·6 vertebrae on the average. When other characters are considered, however, and the variations of this single individual form the averages of the three groups calculated according to the method, we find that

From the average of the White Sea (35 characters) . .	(x^2 or)$v^2=3·213$
,, ,, Vaarsild (35 characters . . .	$v^2=3·696$
,, ,, White Sea (37 characters) . .	$v^2=3·225$
,, ,, Jutland Bank (37 characters) .	$v^2=3·617$

In each case the least value shows that this individual more closely approaches to the herring of the White Sea in spite of its having a seemingly abnormal number of vertebrae. From this it follows that whilst in one character an individual may be very much above the average, it has a variation or group of variations in other characters below the average, which balance by "defect" what the first has in "excess."

The second example taken deals with the group of plaice from St. Andrews. In one respect they do not precisely conform to the conditions necessary for obtaining a "pure" group, in that both male and female are taken. The warrant for doing so is that at the size (12 inches on average) no distinct differences between the two sexes with regard to these characters can be detected. These are compared with the plaice from Grimsby and Aberdeen. The comparison is not made with the total averages of these last two groups, but only with those of two portions which are similar in all respects to one another and similar with regard to size and sex to those from St. Andrews.

It has been known [1] that the plaice of St. Andrews Bay are the young of the plaice which spawn somewhere near the Aberdeenshire coast, and it was therefore of interest to find out if this new method of research would support the testimony obtained by another.

Characters.	Averages for Aberdeen.	$\frac{\Sigma d}{n}$	$\frac{\Sigma d^2}{n}$	Averages for St. Andrews.	$\frac{\Sigma d^2}{n}$	$\frac{\Sigma d}{n}$	Averages for Grimsby.
Intermaxilla (l)	26·80	+ ·4	·16	26·40	·58	+ ·76	27·16
Intermaxilla (a)	21·17	− ·11	·278	21·70	·59	− ·01	20·92
Tail (l) . .	29·56	+ ·23	·467	28·64	2·08	+ ·74	30·89
Eye (b) . .	35·72	+ ·085	·381	36·07	1·57	+ ·49	35·83
Mandible .	46·70	+ ·194	·384	46·07	1·305	+ ·30	45·60
Head (d) .	39·09	+ ·155	·32	39·13	1·43	+ ·02	37·73
Head (b) .	48·72	+ ·01	·38	49·57	2·03	− ·46	47·20
Head (l) .	24·31	+ ·08	·37	23·76	2·06	− ·09	25·28
Body height .	65·18	+ ·16	·40	64·37	2·04	− ·23	63·00
C. Vertebrae .	30·03	+ ·14	·36	30·05	1·84	− ·22	30·07
A. Vertebrae .	12·96	+ ·13	·33	12·95	1·67	− ·19	12·94
Fin-rays P(r) .	11·53	+ ·12	·30	11·52	1·54	− ·20	11·30
„ P(l) .	10·93	+ ·095	·28	11·13	1·42	− ·20	10·91
„ A .	54·28	+ ·086	·26	54·31	1·33	− ·21	53·98
„ D .	72·67	+ ·065	·247	72·91	1·250	− ·22	72·51

St. Andrews varies by ·247 from Aberdeen, and 1·250 from Grimsby.

The characters of this table might have been specially arranged in order to show a uniform gradation of the differences which are tabulated under the headings $\frac{\Sigma d}{n}$ and $\frac{\Sigma d^2}{n}$. But they were tabulated just as they presented themselves, and the fluctuations in these differences instead of being a flaw in the principle only show how necessary it is to take more than a few characters. The small number of characters here taken, and the seemingly very slight differences between the two original groups of Grimsby and Aberdeen, make a severe test of the theory, and thus the results are the more convincing. The values of the deviations between St. Andrews and the other groups gradually

[1] Dr. T. W. Fulton : *Rep. Fish. Board for Scotland*, No. XI., 1893, Part III. p. 176.

become smaller with the addition of every new character, and would continue getting smaller if more were added, because the number n increases faster than the deviations. But the relative proportions of these deviations shown under the headings of $\frac{\Sigma d^2}{n}$ would remain almost constant, as it does from the sixth character onwards. These proportions show that the specimens from St. Andrews are several times nearer to those from Aberdeen than to those from Grimsby. Further, from the columns of the simple deviations, $\frac{\Sigma d}{n}$, we get another important conclusion. The signs of the deviations are different, and this shows that if two curves were drawn to represent the deviations along the same axis of the characters of the groups from Grimsby and Aberdeen, then the St. Andrews group would lie between.

It will have been noticed that instead of calculating the variations for each character, the simple deviations are employed. The reason for this is that when the average deviation is small, less than 1, as it is for the most of these characters, and when therefore the average deviations of both known groups are the same or nearly so, there is very little error introduced by using the deviations directly. In this case if the deviations had been expressed in terms of the probable error the results would have shown larger numbers, as in the case of Prof. Heincke's example, but the proportions between the numbers under $\frac{\Sigma d}{n}$ and $\frac{\Sigma d^2}{n}$ would have been almost the same.

We may turn now from the mathematical to the biological aspect, and however uninteresting the mathematical method may be to most biologists the ideas which it springs from and the conceptions it leads to will certainly be the reverse. Mathematical expressions for the relations between the phenomena presented by living organisms, figures or numbers for the facts of life and the changes in organs, are utterly meaningless in themselves unless the biological standpoint is carefully maintained in the foreground. And it is just in this that one of the chief merits of Heincke's position lies.

If the student of biology brings to his studies a wholesome scepticism of what has hitherto been reported true or false, and yet in spite of his scepticism still retains a strong desire to know and understand things, he will soon come to the conclusion that the manner or method of acquiring knowledge is of as much if not greater importance than the actual knowledge. The phenomena of life, we say, form the raw materials of knowledge, and yet the mind cannot grasp the complex relations and interplay of structure with structure, of organism with organism, and of those with the environment, by entering straightway into the investigation of phenomena, here, there, and everywhere. Some preconceived notions of the subject in hand, and even more, of the right attitude of the observer to the things observed, must be formed; otherwise, however unwillingly, we shall fall into one of two grave errors—either lay stress on the phenomena and pile up detail upon

detail of description and fact without law or connection, or tending too much the other way, show too much of the observer and become guilty of anthropomorphism. The latter attitude is but too prevalent amongst opponents and upholders alike of the theory of natural selection.

The attitude here advocated lies between these two. The details and facts are welcomed, but "facts" do not make true knowledge, and on the other hand, any theory which makes the observer inclined to read into the facts his own personal notions of "utility" or "advantage," for example, must be considered as too prone to misinterpret the actual phenomena. What we wish rather is the attitude of a philosopher who perceives well the facts, but holds himself aloof from opinion, and seeks some method as an intermediary and aid to interpretation. What this method should be is not difficult to conceive.

The outstanding feature in the Darwinian hypothesis—seized upon by its clerical critics at the first appearance of the *Origin of Species*—was the stress laid upon "chance." The conception therein involved was—not that anything ever happened at random or haphazard, but that the changes occurring might be conveniently so expressed. As is well known, this theory of chance has been developed more and more of recent years as a separate study, until the biologist has come to regard it with suspicion as something foreign to his own subject. And yet is there not some truth in this theory of chance? And again, have natural selectionists the prerogative of this truth?

The truth underlying this theory is not far to seek. Life, we may say, depends on many "chances." Hence, knowing the various sources of danger, we may, as if insurance agents, calculate the "chance" of a particular individual surviving to a certain age. In a deeper sense, again, if we knew the causes of variation we should be able to calculate the "chance" of the appearance of any particular variation under certain conditions. What we have before us at any time is only partially the truth, and even if we knew all that had ever occurred and understood all, we—not being omnipotent—could only state as a probability what would occur next.

This is the justification for the theory of "chance," or we should rather say of "probability." [1] Has the theory of natural selection any exclusive right to this conception?

We may judge of this more closely by following the facts grouped under the three propositions stated above. From (1) we learn that the variations in any character are naturally grouped about a certain average, and are usually distinguished as plus and minus variations from that average. If then we hold by natural selection, and maintain that *by this law* evolution may proceed by slow minute steps, so that "even a grain in the balance shall decide which shall live and which shall die,"—which variations shall we call "useful" or "favourable"? If it

[1] A mathematical description of the meaning underlying these words will be found in the "Chances of Death," by Karl Pearson.

is said that we can only tell this by the result, when the "fittest" has proved itself, when the "selection" has already taken place, this simply means that we are unwilling or unable to understand the present. In reality there cannot be two opinions. A plus variation may be "useful" to-day, "harmful" to-morrow, and similarly for a minus one.

It will be said, however, that "plus" and "minus" do not exist in nature, and that the most "useful" or most "favourable" is the mean or average condition. How then shall we understand the continual recurrence of variations from the mean? As shown by Galton, the tendency of successive generations is to produce offspring nearer the average than the parents, and hence, on the theory of the survival of the "fittest," or most "favourable" variations, we are unable to explain the presence of extremes.

Natural selection is based on (1) the rate of increase of offspring, (2) enormous destruction and "struggle," (3) survival of the best fitted to the conditions. If we let our minds run smoothly in the train of thought suggested by the form of the premises, the conclusion is inevitable. But if we inquire more closely into the "struggle" and "destruction" in any particular case, say of the eggs and young of the herring in the sea, we find ourselves obliged to consider this destruction as indiscriminating and independent of struggle, and that consequently both the "fit" and "unfit" survive; in other words, we cannot apply these latter words even "metaphorically."

If natural selection thus fails to interpret present phenomena as shown in the variations of single organs, its difficulties increase when we consider the individuals. From the study of organs we might conclude that Nature was aiming at the conservation of the average, but when we examine many organs we find that the average of one may be combined with the extremes of others. Hence greater fitness in this or that has to make up for greater unfitness here or there. In the same region the individuals of a group at the same period of life are thus equal in the combination of their characters. This has been shown to be a theoretical deduction from the first proposition, and it has been exemplified under propositions II. and III. Any conception of greater or less fitness is here completely excluded.

When we turn, however, to different regions and consider different groups of the same species we find that the average of the individuals has changed, and if we examine successive groups in successive regions we find intermediate stages of the averages. It might be thought, then, that natural selection has brought about these differences in the different regions. If so, then we must change our conception of natural selection because there is little or no "struggle" between the different groups, consequently no discriminating destruction and no "survival of the fittest."

It is not to the present purpose to criticise further the theory of natural selection, or plead for the theory of probability. This latter,

we see, can be divorced from the former, and in its making for accuracy is in truth but the expression of rationalised scepticism. Let us turn to certain of the conclusions to which Heincke has been led by employing the theory.

The variations that we find conforming to the laws of mathematics cannot be considered as the "beginnings" or "makings" of new varieties and species; they are the actual condition of affairs, the product and reflex of the varying elements of the environment. These variations in structure are found to show the same appearance year after year, and thus a similar curve of variations will be obtained year after year although the individuals examined are of different generations. Similar curves are obtained for different groups of the same species taken from different regions although the mean or average value is changed. If we look to the environment we see that the conditions there of temperature, salinity, etc., present similar curves. We are surely entitled to connect these two sets of variation and state that the one gives rise directly to the other.

The variations in organs which are of specific importance are for the most part formed in the early stages of ontogeny when the organism is plastic and sensible to the fluctuations in the environment. The range of variations is represented by the "variation coefficient," the "probable error" of the variations. This is not exactly the same for different regions, and hence forms a means of comparing the ranges of the variations in the different environments. It also represents the average capability of varying which each group possesses with regard to the particular organ, whilst the total range of deviations actually observed represents the variability for the group, and thence of the species. The importance of the conception is evident.

The individual as a combination of organs is, however, the turning-point of the position of Heincke. Reference might be made to eminent biologists who have expressed similar ideas, but perhaps Herbert Spencer comes the nearest. His conceptions of "life," the "balance of organs," "direct and indirect equilibration," are almost exactly repeated by Heincke in other words. But whereas Spencer took a broad view of the problems of evolution, and thence showed the various factors in perhaps their true perspective, Heincke has concentrated his attention on the meaning of certain carefully observed facts.

Each individual of a group is the chance combination ("permutation") of a number of possibilities, each combination being equally probable. Hence the individuals at the same stage of life, whether as larvae, young, or adults, possess an equal "balance" of the possible variations of their organs. And similarly for other groups, the destruction that occurs is a destruction of combinations of equal value with one another and with those that survive. The survivors give rise to further combinations, each within the same range of variability as before, and each equally probable, the exact combination and balance

depending upon the immediately surrounding conditions. Thus, whilst each new individual represents a fresh combination of the "possibilities," the mingling of the sexual elements is the "dissolution of the chance." Death is not the giving place to the "more fit," nor the resignation of the individual for the good of the species, but the natural ending that comes to all by whatsoever "chance."

"Utility should therefore be replaced by probability." The individuals of the race start out on life with the same opportunities and capabilities, and the probability of the so-called "success in life" is inversely proportional to the "chances of death." By man's agency these latter may be and are continually being altered, and thus give rise to appearances which have formed the foundation-stone of natural selection. But apart from man it is permissible to conclude from the balance of things that the chances of success in life are in exact inverse proportion to the chances of death. If the conditions of life were continually recurring, therefore, with periodic regularity, the chances would be practically constant and the "balance" of the individuals would remain the same. But when the conditions of life change and the change remains constant the balance of the organs in the individuals is altered. Thus, whilst the adults may or may not be affected—for the chances of death may be increased or diminished—the combination of organs in the youngest must alter, and from this directly-caused change new races will arise.

A fine distinction, but an important one, should be noted here. Darwin observed that species the most widespread and most abundant varied the most. This, however, does not truly represent the case. As we pass over the different regions inhabited by the species we see that the diversity in characters is greater than in a smaller species. In reality the "variation," *i.e.* the variation-coefficient in the individuals of the different regions, may be exactly the same for all, where that which is characteristic of the larger species is its greater "variability."

From this aspect of variability we can understand how the individuals of a group differ from one another in all parts of the body and at all stages of development. No two individuals are "alike," though all are "normal" and equally "fit." This variability shows itself not merely in the earliest stages, but through every stage of development for many characters. Hence we get differences due to growth or age, the "balance" of the different organs not remaining the same throughout.

From these fundamental positions Professor Heincke passes to a criticism of systems of classification, and to suggestions for a new and better one. The older systems have begun with orders and classes and worked down towards species and varieties. This method has succeeded for the orders, but not for the species. Hence we must begin at the other end, below the "species," and work up to the orders.

The first group, therefore, is the Race or "Stem" ("Family").

This includes "those individuals which live within a certain region, under equal conditions, have the same habits, and stand in close blood-relationship through intercrossing and reproduction." The characters of these individuals will come under the laws formulated here as propositions II. and III.

The older systems regarded the above as the species, but in a natural system the "species" is the second group. It is a certain "combination" of races whose exact limitation has to be determined in all particular cases. The species is but a larger "race," and may be sharply marked off from or merge into the races of another species. The sharper the races can be marked off from one another the clearer will be the distinction between the species.

As this paper is so short, it is hardly necessary to make any summary. The endeavour has been to display the meaning and importance of Heincke's work, and if further information is desired reference should be made to the original work.

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A Zoologist on the Principles of Science.[1]

By F. A. Bather, M.A.

Professor Brooks entitles his book "The Foundations of Zoology," but he ends by referring to it, more justly, as "my work on the Principles of Science." It is not modesty that selects the less comprehensive title. It is desire to emphasise the belief "that the principles of science, as distinguished from the concrete sciences, are part of biology."

The ground taken is that the methods of extending knowledge, as well as the generalisations therefrom, are operations of the human mind. Every physical science rests on a metaphysical basis, which has its origin, so far as we are concerned, in the mind. It is the student of animals who has to face the problems presented by the origin of the mind; and it follows that, when questions are raised concerning the operations of the mind in scientific study, "the zoologist has a peculiar right to ask answers, in addition to the right which he shares with other students of science." Or, from another point of view, since life is response to the order of nature, the study of the order to which response is made is as much a part of biological study as is the organism which responds.

When I found this to be the author's conception of his office, a sense of incompetence urged me to withdraw from the attempt to deal with matters so profound. But the description of the book as "a course of lectures delivered at Columbia University" reassured me: at least I was capable of learning. We students of zoology need a book to show us the relations of our science to broader schemes of philosophy, a book written from our own standpoint and condescending to our ignorance. The fact that this need is not recognised by all of us merely shows how real a need it is. And here, perhaps, lies the chief value of the present work. Dr. Brooks has so deserved a reputation as a zoologist, that any writings by him on "the foundations of zoology" are sure to be read by his fellow-workers; and through these essays their interest will be stimulated and their intellectual sympathy widened. But as "a course of lectures"! Well, the least

[1] "The Foundations of Zoology," by William Keith Brooks. Columbia University Biological Series, vol. v., 8vo, pp. viii. + 340. New York: The Macmillan Co. London: Macmillan and Co., Ltd., 1899. Price 10s. 6d. net.

that can be said is that the students of Columbia University have not been fairly treated. Some old lectures "prepared at different times and for various reasons" have been furbished up and intermingled with extracts from reviews and other magazine articles. The almost unavoidable consequence is superabundant repetition, not always free from inconsistency, a want of coherence, not wholly remedied by an interjected paragraph or two, an absence of logical arrangement and continuity in the development of the main thesis, and long complicated sentences to be attacked only by the midnight reader with a wet towel.

Despite these defects, the conclusions or leading ideas of the book, if not simple, are few. In fact the author states that his sole purpose is to show that mechanical conceptions of life and mind cannot make right deductions from true principles untenable (p. 29). This statement, however, scarcely illustrates the scope of the work, and the reader will doubtless wish to know what those particular deductions may be that Dr. Brooks holds to be proof against all attack. I shall therefore attempt a brief relation of the leading ideas in the book.

The two fundamental conceptions that appeal specially to the biologist, and are in large measure the outcome of his labours, are the principle of genetic continuity and the principle of fitness. Significant resemblances recognised between the phenomena of nature may be due to genetic continuity; and the order of nature may be the order of fitness.

The meaning attached to fitness by Dr. Brooks is at once seen in the second Lecture, entitled "Huxley, and the problem of the Naturalist." It is mainly a criticism of Huxley's essay on "The Physical Basis of Life," and its keynote may be thus expressed.—Admitting that protoplasm is the physical basis of life, and even supposing that its properties are a result of its molecular structure, still life is not one of those properties, but the adjustment of the properties to the environment, so as to promote the welfare of the species. As Aristotle put it, the essence of a living being is not what it is made of or what it does, but why it does it. The problem of the naturalist is therefore the study of this adjustment; in other words, the problem of fitness (p. 39). Later on, however (p. 246), we are told that "the problem of the naturalist is not the existence of adaptations as such, but the existence of adaptive species." The limitation will be found important.

The problem stated, we proceed to its consideration; and the next three lectures deal with one of the proposed solutions, that of Lamarck, and the so-called Neo-Lamarckian emendation of it. "Stated briefly," and, I think, fairly, "it is the doctrine that organic evolution has been brought about, or at least greatly aided, by the inheritance of nurture." By "nurture" we are to understand all manner of modification due to the external world.

To this doctrine Professor Brooks raises an objection that seems to have an insecure foundation. We all admit a present fitness in the

organic world, by no means an absolute fitness, but enough to call for explanation. We also admit that living individuals are capable of nurture. But, says our author, the nurture may be good or bad, quite as often the latter as the former. Therefore "the view that nature is inherited nurture throws no light on the problem of fitness." Accepting the premises, and setting aside natural selection and other factors, we might grant that unfit modifications would so counterbalance fit modifications that the conclusion would follow. All turns on what is meant by good and bad nurture. Nurture is, broadly speaking, the influence of environment on the individual. Now environment can be called favourable or unfavourable only from a relative standpoint; that is, so far as the individual is or is not adapted to it. We know no absolute good, no ultimate morality. As Dr. Brooks elsewhere says, "no natural response can be beneficial under all circumstances"; education and experience (which, be it noted, are forms of nurture) enable organisms to distinguish the harmful from the beneficial occasions (p. 13). It is admitted that we start with individuals fairly adapted to their environment, and that change in the individual or the race is induced by change of environment. But it is clear that any change of environment breaks the harmony and must be unfavourable to the individual: natural actions are beneficial only "so far as the environment is, on the average, like the ancestral environment" (p. 10). The change continues unfavourable until the individual or the race is modified in accordance with it; but this modification is itself beneficial only so long as the same change persists or continues in the same direction. Nurture is found to have been "bad," when the change of environment has been only temporary or extraordinary. Man, subject as he is to so many and great changes of environment, is often led into surroundings or habits at variance with the general conditions that govern the existence of his race: these things we rightly call "bad." But with other organisms and in physical nature changes of environment are, as a rule, secular, and proceed equably in a certain direction. Therefore their action on individuals is regular; in other words, the nurture is "good" on the whole. But if it be conceded that the good preponderates ever so little over the bad, the objection of Dr. Brooks becomes invalid.

Other considerations advanced by our author may render the Lamarckian doctrine unnecessary or less probable; but I fail to see that they prove the inheritance of modifications to be either impossible or ineffectual.

The first consideration is the truth of natural selection and its adequacy to account for animated nature as we see it. Most Neo-Lamarckians admit natural selection, though not its complete adequacy. But even that might be admitted without diminishing the adequacy or effect of any other factor. It is hardly necessary to point out that, under any theory of heredity and development, the rate of progress

through selection will be proportional to the number of variations in the direction of progress. If variations be governed by the laws of chance alone, that number must be less than when variations are determined in the direction of the environment by the inheritance of modification. Fitness would be reached more readily if modifications were inherited.

The second consideration is more subtle. It is a question whether education or the action of the external world can add anything to the nature of the organism, whether it does not merely unfold and develop the original nature. Here is the old difference between development by epigenesis and by evolution. Dr. Brooks makes a compromise that seems consistent with common sense. The organism, he says, would not develop without the education, but the character of the development is due to its original nature (p. 15). No vital action takes place without a stimulus; but the stimulus is one thing, the character of the action is another, and is dependent on the nature of the organism. Thus in ontogeny each change may be called forth by some mechanical stimulus, either within the body or without, and yet the nature of the whole may depend on the nature of the germ (p. 59). "External conditions press the button, but it takes all the inherent potency of living matter to do the rest" (p. 61).

An ingenious application of this conception may be noted in passing. It is that "organs once adjusted to the external world may, after the adjustment has lost its meaning, be still retained, because they furnish physiological stimuli, which excite developmental changes in the organic mechanism" (p. 10). Thus Dr. Brooks accounts for the retention of so-called rudimentary organs and recapitulatory stages.

But we have to see how the conception affects the problem of fitness. If it be correct, if, in other words, nurture adds nothing to nature, then there is nothing to be inherited. But the problem does not become easier of solution. It consists of two parts: the adaptability of the individual; and the adaptation of the race. The adaptability of the individual resolves itself into the adaptability of protoplasm, and none is so bold as to say he knows the explanation of this. Turning to the adaptation of the race; each new germ would, on this conception, be similar in all respects to the primordial protoplasm, being in fact nothing but an extended part thereof, but gradually becoming more and more gifted with the power of growing into a being modified in accordance with its environment. But it is very difficult to see why or how it should obtain this power, except through education. The faculty of being educated was, we may suppose, present in the original protoplasm; and it has gone on being educated ever since. Some portions of it, from one cause or another, did not respond so readily to education, and they have been expelled in consequence; that is what we mean by natural selection.

True it is that this way of looking at the case brings in the direct action of the environment just as much as ever, and that everything depends on this and on the fundamental properties of protoplasm, or, if you will, living protoplasm (we know no other). Thus, by accepting the contentions of Dr. Brooks, I am led to the very position he is trying to attack.

Here seems the place to allude to two passages much further on in the book (p. 187). "A living thing is a being which responds to the stimulus of one event in such a way as to adjust its actions to other events of which the stimulus is the sign, and as all that have not thus responded have been exterminated in the struggle for existence, the adjustment of the survivors is no more than might have been expected." "They who assert that it [natural selection] is inadequate because it fails to show why beneficial response should ever follow a stimulus, and thus furnish fitness to be selected, must remember that all science is inadequate to exactly the same degree."

These sentences seem to imply that the fitnesses or adjustments selected are the outcome of response to a stimulus, and not merely response, but beneficial response, *i.e.* response in harmony with the environment. We do not know and need not know the why or how of this responsive faculty; it is to be enough for us that it is a property of living things. It is not clear how this differs from the following statement by another author:—"All adaptations, at any rate all adjustments concerning whose action and efficacy there is no dispute, have arisen in the same way as the enlargement of a muscle by exercise," *i.e.* as beneficial response to a stimulus; and this faculty of response is "a fundamental property of protoplasm" (Cunningham, *Nat. Sci.* vol. viii. pp. 328 and 330; May 1896). But there must be a difference, for these are assertions which Dr. Brooks combats with abundance of sarcasm.

Perhaps the explanation is that we have here "a bad and unapt formation of words." "Adjustment" seems to be used in two senses: the act of adjusting and the result of adjusting. Just so Professor Brooks sometimes uses "*nurture* instead of *acquired characters*," whereas the latter are elsewhere more correctly spoken of as "the effects of nurture" (pp. 55 and 172). Since Dr. Brooks and the Neo-Lamarckian both admit (I believe) the operation of natural selection, the difference between them seems to lie in this: that, according to Dr. Brooks, the faculty of adjusting is a character that varies and is selected and inherited; while, according to the Neo-Lamarckian, the results of adjusting are the characters that are selected and inherited. It is, however, clear that selection can act on the faculty of adjusting, only through its concrete results. Further, no human being can perceive whether the faculty of adjusting is transmitted, except by seeing the results. But in the ovum these results will not be manifest to the most keen-eyed microscopist; like all other characters, they will appear

gradually. Who then shall decide whether they are the results of adjustment *de novo* in each case, or whether they are the inherited results of prior adjustment? So far the history of the controversy has shown every test-case to be capable of two interpretations.

In Lecture IV., "Lamarck," a third consideration is brought forward. Dr. Brooks tries to show that inheritance of modifications, even if admitted, would not produce such a world as we know. This he does by citing a number of instances in which the modifications affect other species (*e.g.* the bee's sting, the serpent's poison), or other individuals than the ones exhibiting them (*e.g.* the rabbit's white tail); also modifications for the good of the species, occurring only in non-reproductive individuals, and therefore incapable of inheritance. He maintains that "*in all cases* the structure, habits, instincts, and faculties of living things are primarily for the good of other individuals than the ones that manifest them" (p. 88); "there is nothing anomalous or exceptional" in the instances which he selects. This does not mean that the serpent's tooth is useful to the rabbit, or that bees sting us for our moral edification. None the less it is a hard saying, and difficult of application to such protective structures as the carapace of the tortoise, or to such (apparently) useless characters as baldness.

In the vast majority of instances this "general law" can be nothing else than that an organism has such structure, faculties, etc., as enable it to produce offspring. But we are told, *every* character is *primarily* for the good of others. Senile characters, which, as in the Ammonites, appear ever earlier in succeeding generations, may be explained as due to the direct action of the environment, or perhaps in some roundabout way by natural selection. But imagination boggles at the idea that they were of use to offspring born long before the characters appeared.

The deeply interesting Lecture V. adduces migration as instance of an action for the preservation of the species, but often leading to the loss of the individual, *i.e.* an action for the good of others, and therefore not explicable on Lamarckian principles. But though natural selection be admitted, no multiplication of similar instances can disprove the operation of the Lamarckian factor.

Lecture VI. attacks the evolutionist philosopher, he who holds not merely that the universe has evolved, but that its evolution in that particular way was a necessity from the beginning, and that all was latent and determined in the primal nebula. Not that this philosophy may not be correct, but that it is, as Huxley said, premature.

A note shows the fallacy of Galton's and Weismann's view that the ancestors of an individual are doubled for each generation that one traces them back. The fallacy lies in the omission to recognise the almost inevitable inter-breeding.

Lecture VII. continues the criticism of Galton. His data "fail to prove that the 'principle of organic stability' owes its existence to anything except past selection; that regression to mediocrity occurs

when *ancestry* is studied uncomplicated by *nurture;* that the 'mid-parent' is anything else than the actual parent; that 'sports' are fundamentally different from the ordinary differences between individuals; or that natural selection is restricted to the preservation of sports" (p. 178). Galton's statistics are no evidence as to the effects of inheritance, because inheritance is of many characters, not of one only; and in the statistics the effects of nurture are not sifted out.

Lecture VIII. defends the pure natural selection theory of Darwin against two criticisms.

First, that, since natural selection "does not produce, but only preserves the fitness which exists, it does not show why there should be any fit to survive, but only why the unfit are exterminated." Dr. Brooks says "the statement that selection could not act unless they [the useful variations] existed is childish" (p. 184). Why? Because "it is obvious." Very well! then it is equally obvious that natural selection does *not* "account for the whole history of" any character Granting variations, natural selection is an adequate explanation of the origin of species. But this is not the same as the evolution of our present fauna and flora from a protoplasmic slime. Childish though our curiosity may be, we cannot close our minds to the questions: Why does living matter vary? how is it that variations are inherited? is there a limit to variation other than that imposed by physical conditions? with many other questions that have nothing to do with the operation of sorting into species, but which call for answer before we can understand the mode of organic evolution. Because Darwin, as Dr. Brooks justly urges (p. 187), wrote for a certain set of readers and on a certain problem, this is no reason why we are never to proceed beyond that problem.

The second criticism is that many differences can have no selective value in their incipient stages, whether of organic structure, as instanced by Mivart, or of mental action, as instanced by Romanes. This objection can only be met by dealing with each instance in turn, and showing that it has selective value.

Lecture IX., "Natural Selection and the Antiquity of Life," appeared in the *Journal of Geology*, and was dealt with in *Natural Science* for October 1894.

The remainder of the book considers the evidence for purpose in nature, and first the argument from design as given by Paley. This is stated in two forms. First (p. 258), "(1) Nothing accounts for watches but mind. (2) Nothing accounts for living things unless it accounts for watches. (3) Nothing but mind accounts for living things." This is most obscure, and the phrase "accounts for" is ambiguous. If it means "is the sole cause of," then the minor premise begs the question or else is absurd. If we interpret (2) as, "The cause of living things involves the cause of watches," which is true, then the only conclusion is, "The cause of living things involves mind," even as it includes

many other things, which are the results of life, not its cause. The second statement of Paley's argument has for its major premise "Evidence of usefulness is evidence of design." But this also is just what has to be proved, and it certainly cannot be laid down as a universal proposition.

But it is late in the day to be discussing the logic of the argument from design, and it is only for sake of reference that I venture on a more complete statement:—(1) Watches and the like are admirably fitted for a useful purpose. (2) They *are* the result of design. Therefore (3) other things admirably fitted for a useful purpose are *probably* the result of design. (4) The structure of living things is so fitted. (5) Therefore they are evidence of design. Design in every case is understood to imply a designing mind. This cannot carry conviction. Premise (2) is an induction liable to be upset by a new fact. Proposition (3) is an argument from analogy only. Premise (4) can only be proved by the accumulation of instances.

The argument, in short, is one of probabilities, and the important question is how far those are affected by the acceptance of natural selection.

Dr. Brooks rightly remarks that "the mere extension of the domain of natural causation," "the demonstration of the mutability of species," in a word, evolution by descent, cannot give a blow to the argument. The fitness is not thereby affected, and the conception of design appears even more necessary.

Nor does the substitution of physical causes in place of special creation weaken the inference. The fitness remains, however brought about; and there is no reason why the designer should not work through physical causes.

Huxley is represented by Dr. Brooks as saying that there is a wider teleology which is untouched by natural selection; but his words are—"not touched by the doctrine of evolution"—a very different matter, as Huxley always insisted. What he did say about the Darwinian theory was that it was absolutely "opposed to teleology as it is commonly understood." Since the commoner teleologists agreed with Huxley in this opinion at least, it is fruitless for Dr. Brooks to raise objections. How far the more subtle teleologists may be affected is another matter.

The new aspects of the case introduced by admitting the all-potency of natural selection seem, put baldly, to be these:—Our proposition (4) ceases to be true; since natural selection implies that for every individual which is fit and persists, a hundred or a thousand are unfit and perish. This is a different idea from the absence of perfection in those selected, which would not invalidate the argument from design. Applying the teleologist's favourite analogy, the present point may be enforced in two ways, thus: that a designer 99·9 per cent of whose plans are rejected has chosen a wrong profession; or that if of a

thousand stones one happens to fit a hole in the wall, this is no proof that it was shaped with that hole in view.

Next, the probability of proposition (3) is lessened. The argument from analogy loses much of its force. Watches may be good or bad, but from the beginning they have been made with the express design of measuring the divisions of the day; to reach this result, obstacles are overcome and portions of the physical universe bent to the will of the designer. On the theory of natural selection, the evolving species presents none of these features; we deceive ourselves when we see in the Palaeozoic brachiopod signs of the direction in which its Mesozoic descendants will evolve; obstacles if presented are not overcome, but cause the line of evolution to swerve; no part of the physical environment is controlled by the species to its good, but the history of the species is controlled by the environment.

The argument from design, as stated by the older teleologists, seems to be seriously weakened by the theory of natural selection as stated by Darwin. On the other hand, that same theory may, as Dr. Brooks shows, enable us to restate the argument in a more convincing manner. First it is to be noted that all human contrivances are subject to natural selection in the same way as are the contrivances of other animals and as the animals themselves. This strengthens the analogy, but does not render it anything other than analogy; for liability to natural selection is no proof of similarity in other respects. Natural selection, to adopt a phrase dear to Dr. Brooks, shows how things happen, but it does not show why they happen.

Further consideration reveals a more fundamental change in our ideas. "The modern zoologist," says Dr. Brooks, "must ask whether we are sure that nothing but mind accounts for watches" (p. 259). "The progress of zoology has forced us to ask anew the old question whether a watch may not be part of the chain of physical causation just as truly as the spider's web or the cat. . . . The discovery of natural selection has put the matter in a new light" (p. 264). If the suggestion be admitted, "Paley's analogy" does indeed "become impregnable," but his major premise (that mind is the cause) becomes the proposition to be proved.

The problem remains, but must be attacked in another way. Whatever be the explanation of the phenomena of life, there is no doubt as to the usefulness of those phenomena to the living beings that exhibit them; and in this lies an apparent distinction between living and not living things. This faculty of using or controlling portions of the physical world, a faculty which Dr. Brooks chooses to express by the word "contrivance," may be regarded as "interference with the order of physical nature" (p. 273). Without following Dr. Brooks in his discussions of personal identity and spontaneous generation, we may agree to the continuity of life, and must admit that this faculty is coextensive with life. Now natural selection shows us the

"chain of physical causation which joins the works of man and of other living beings to that part of the order of nature to which they are adjusted." The remaining question is whether life itself and all its faculties are purely physical phenomena, or contain something which cannot "be expressed in terms of physical matter and mechanical energy." If purely physical, then usefulness and contrivance are a part of, and not interference with, the order of physical nature; and living beings can "afford no peculiar evidence of purpose." But the question is not yet answered, and the argument from contrivance, though its probability is vastly lessened, has not yet received its death-blow.

But, supposing the mechanical conception of life to be established, and admitting that the argument from contrivance would thereby lose its force, the attempted proof of the existence of a designer would not on that account be supplanted by disproof. Further, whatever the scientific account of nature may ultimately be, it can throw no light upon the primal cause or final purpose of the whole or of any part. Science tells us what takes place, and how it takes place, she discovers the succession of events and gives us a reasonable confidence in the steadfastness of that succession, but she refuses to admit any necessity therefor, and as to any cause that lies behind the veil of the physical universe, she remains for ever dumb.

But, though the scientific method may throw no light on anything beyond the facts of nature, it is still open to inquiry whether the consideration of nature as a whole may not throw some light upon the ultimate cause. Thus we are led to the conception finely expressed by Oerstedt in the phrase: "The works of nature are the thoughts of God." This view has been elaborated by a great philosopher, Bishop Berkeley, and by a great naturalist, Louis Agassiz. Each in his way maintains that the phenomena of nature constitute "a *language* in which the Creator tells us the story of creation for our delight and instruction and advantage." But each weakened his case and lost the adhesion of modern naturalists, because, as Dr. Brooks insists, he attempted to prove too much. Agassiz thought it necessary to show that the laws of nature were *nothing but* categories of thought, that they were arbitrary, and that no physical explanation of them was possible. Berkeley wrote: "The great Mover and Author of nature constantly explaineth Himself to the eyes of men by the sensible intervention of arbitrary signs, which have no similitude or necessary connection with the things signified." And in another place he held that this language of nature was necessary to assist the governed. But the modern naturalist is aware of many a physical explanation unknown to Agassiz; he sees more connection between the sign and the thing signified than was possible for Berkeley; and he refuses to admit any necessity in the matter.

But a conception supported by indefensible arguments is not

necessarily false. "As I understand Agassiz," says Dr. Brooks, "it is not because natural history is a language that he holds it to be intended; but because it is delightful to listen to the language of nature, and because it abounds in beneficial instruction for mankind." And again: "As I understand Berkeley, it is not because nature is orderly, but because the order of nature is useful, and instructive, and full of delights for living things, that he holds it to be a language." Let us admit that response to nature and the study of nature are all these things; it does not therefore follow that the language is necessary or unnecessary, and I do not see how it follows that the language is intended. It may be so, but, on the other hand, "the modern zoologist must also ask whether natural selection, so far as it accounts for living things and their works and ways, does not in the same measure account for language; both that which men use among themselves and that which we find in nature" (p. 337).

We close the book, then, as ignorant of fundamental truths as when we opened it. But we have now reasons for our ignorance. Professor Brooks, in so far as he has adhered to his maxim—"The assertion that outstrips evidence is a crime"—has convinced us of his main thesis, which indeed is a corollary of that statement, and may be expressed in the words on the wrapper of this Review:

Nunquam aliud natura, aliud sapientia dicit.

British Museum
(Natural History),
London, S.W.

On the Multinuclear Cells of some Grasses.

By RUDOLF BEER.

Plates I. and II.

A FEW years ago we unhesitatingly affirmed that the cell was the ultimate unit of the animal or vegetable body. At the present day we do indeed continue to uphold the cell as the elementary structural component of the living body, but not without some misgivings, since recent research has made us acquainted with various phenomena which we find it difficult to reconcile with this conception.[1]

The work of a large band of investigators has shown that in a number of tissues, both animal and vegetable, the intercellular wall forms by no means so sharp a separation between the protoplasmic bodies as was formerly believed. In these cases the refined methods of modern research have revealed to us a system of delicate fibrils of protoplasm which pass through perforations in the cell-wall, and directly connect neighbouring cell-bodies.

Further cause for uncertainty has been given by the discovery of multinuclear cells, *i.e.* cell-cavities which contain a single protoplasmic body in which are included a plurality of nuclei. Among the lower plants we meet with whole groups of organisms (*e.g.* Siphoneae) in which the body shows no septation into cells, although it is frequently both large and highly differentiated.

To use Sachs' phrase, the bodies of these plants are "non-cellular," for they contain a large, continuous mass of protoplasm which is studded with innumerable nuclei (15[2] and 16).

The embryo-sac of the higher plants, at one time of its existence, contains a large number of nuclei (8); the laticiferous tubes of a number of plants (Euphorbieae, etc.) are also multinuclear (18); the elongated bast-cells (18), the cells of the suspensor of some Leguminosae (6), the older internode cells of Characeae (10), the older parenchyma

[1] A full explanation of the terms used in this paper will be found in two articles, by Mr. Hill and myself, which were published in previous numbers of this journal (7 and 1).

[2] These numerals refer to papers, etc., quoted at end of article.

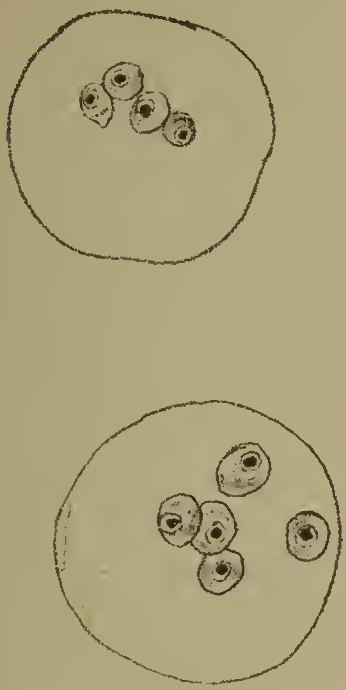
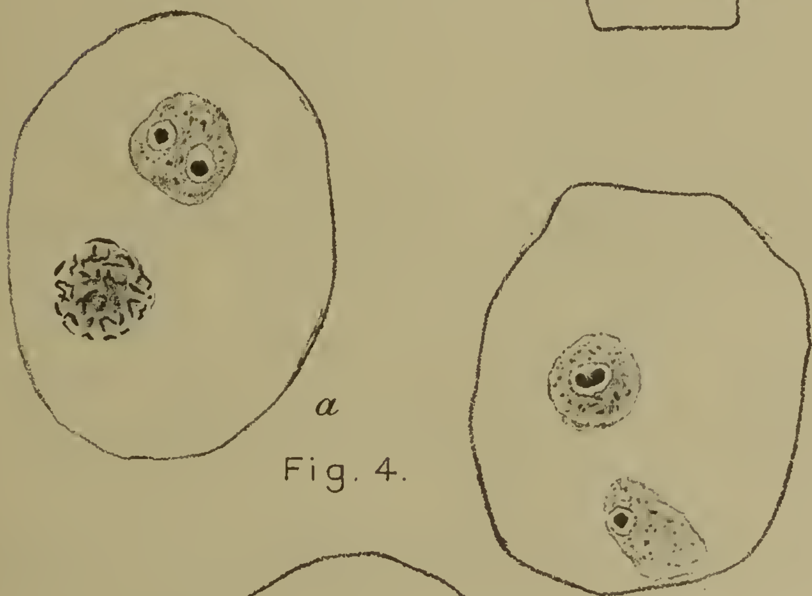


Fig. 1.

Fig. 2.

Fig. 3.



a

b

Fig. 4.

Fig. 5.

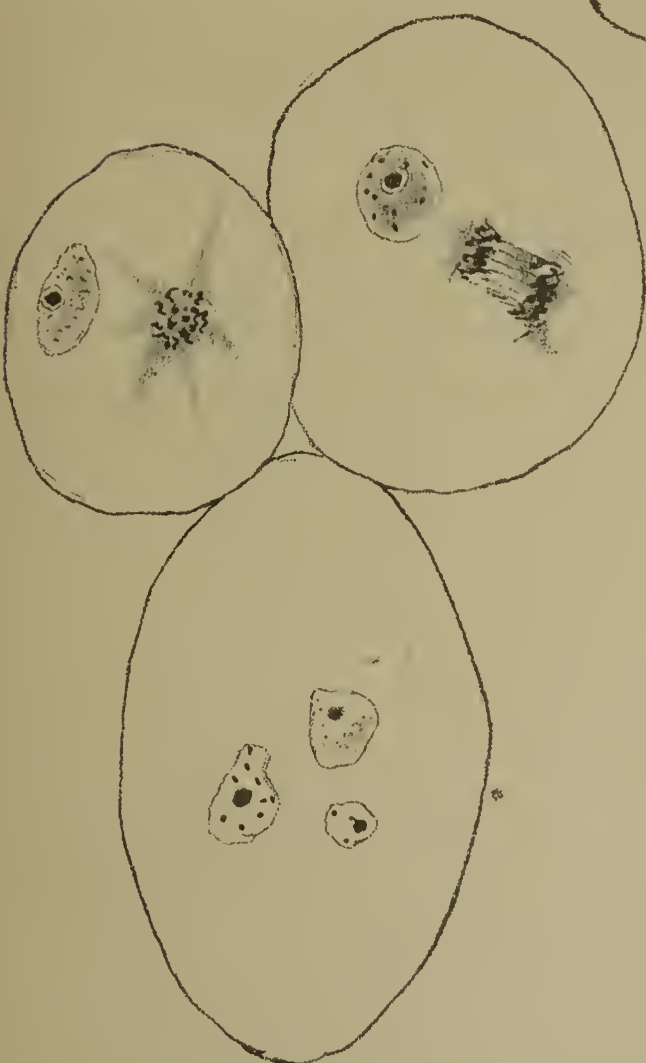


Fig. 6.

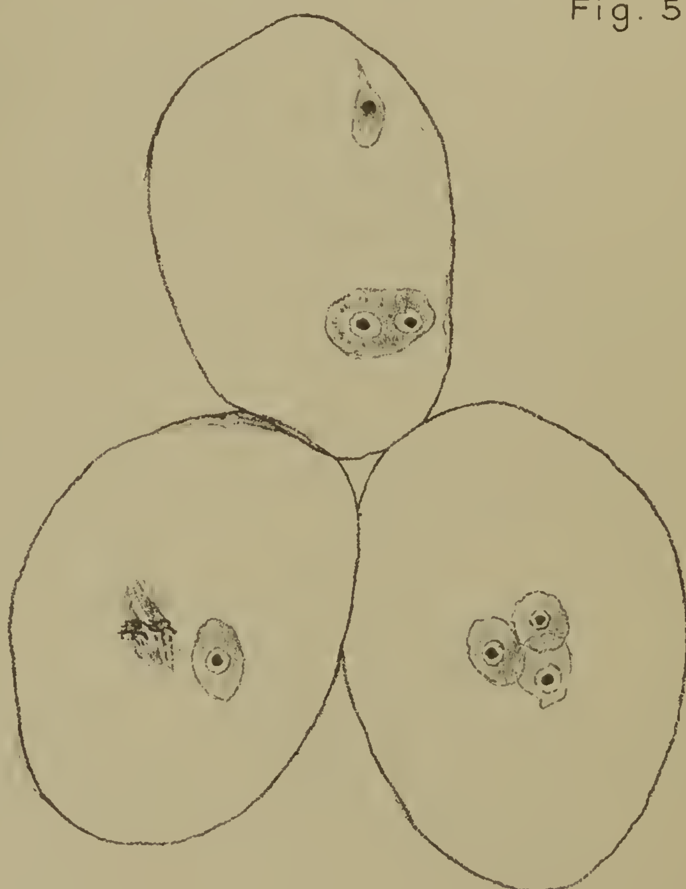


Fig. 7.

Fig. 8.

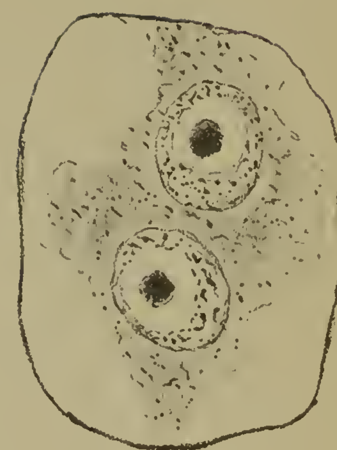


Fig. 9.

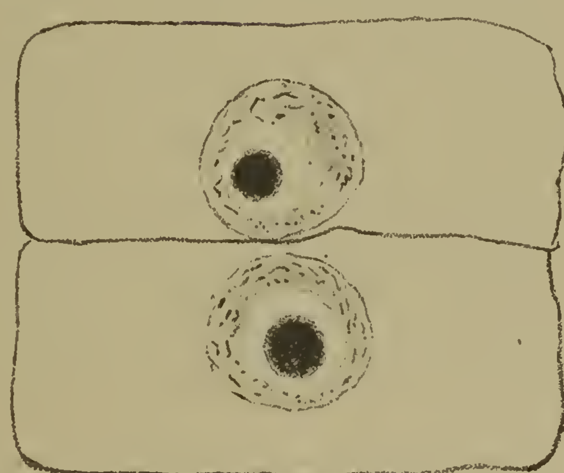


Fig. 10.

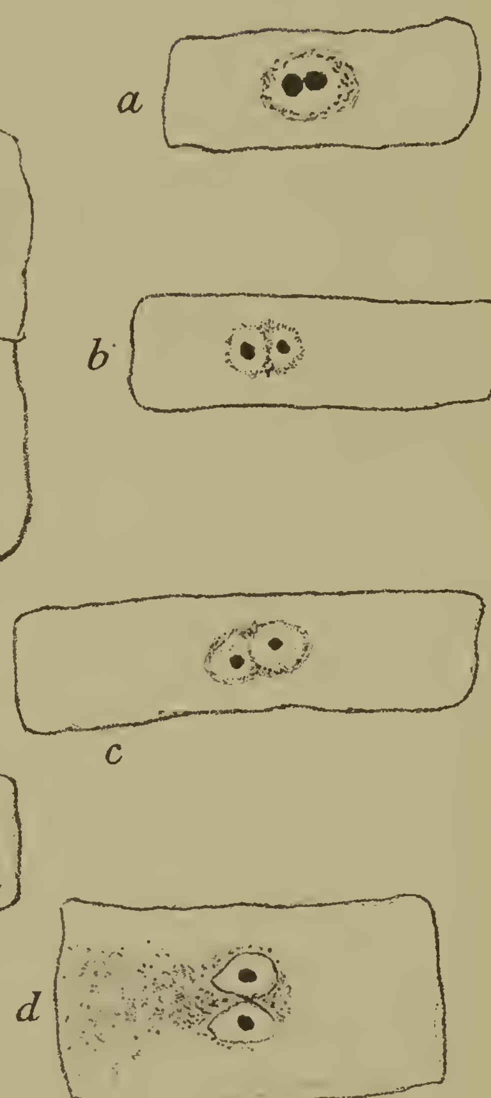


Fig. 12.

Fig. 11.

Fig. 13.

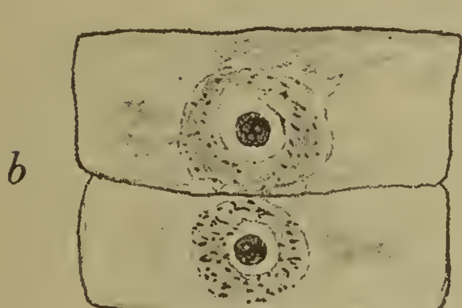


Fig. 11.

Fig. 13.

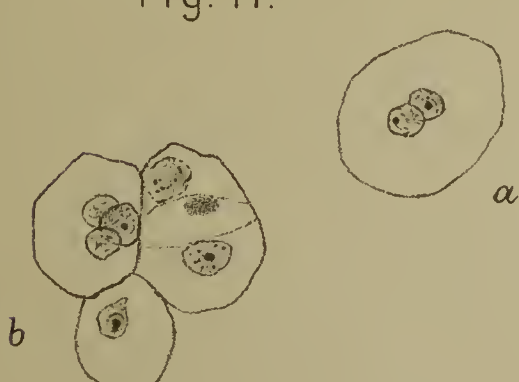


Fig 15.

Fig. 14.

cells of many Monocotyledons (9), the tapetal cells in the sporangia of *Angiopteris* (4), the generative cells of the vessels in Dioscoreaceae (3), the older parenchyma cells of *Taraxacum officinale* (15), the large parenchyma cells of *Cereus multangularis* (18), the young, elongated pith-cells of *Ochrosia coccinea* (18) have all been shown, by various observers, to be furnished with a plurality of nuclei.

All our experience teaches us that wherever a number of nuclei appear (whether these be sooner or later separated by a cell-wall, or remain together in a multinuclear cell), they arise from the division of an original mother-nucleus. When a nucleus divides into two daughter-nuclei, it does so by one of two ways.

Either it becomes constricted here or there, and without more ado breaks into two or more parts, or it first passes through a complicated series of preparatory stages in which certain of its internal parts describe the most curious "figures," and then only separates into two daughter-nuclei. In the former case the division is said to be *direct* or simple fragmentation, in the latter it is described as *indirect* or karyokinetic (7). It is generally supposed that a nucleus which is fragmenting has lost the power of dividing activity by karyokinesis. The great German cytologist, Strasburger, writing in 1880, says: "According to my entire experience karyokinetic division and fragmentation cannot be brought together, and *certainly one cannot replace the other*" (17).

Recently, however, the Italian observer, Buscalioni (2), has shown that this separation of the two forms of division is by no means necessarily the case, and that in the development of the embryo-sac of *Vicia Faba, Lupinus, Fritillaria imperialis,* and *Leucojum vernum,* and in the laticiferous tubes of *Urtica,* fragmentation and karyokinesis may take place side by side with one another, or the same nucleus may first divide directly and then indirectly. Moreover, both Buscalioni and Dixon (5), as well as Miss Sargent (14), have observed a curious condition of the nucleus in which some of the preparatory stages of karyokinesis are gone through, but before the process is complete the nucleus divides directly. Whether this is really an intermediate stage pointing to the fundamental identity of the two processes, as the authors apparently suppose, is doubtful. The facts clearly indicate, however, that the two varieties of division are by no means incompatible with one another (19).

Some observations which I recently made on certain vegetative cells of some Gramineae give additional support to this view.

It does not seem to be as widely known among botanists as it should be that in certain members of the Gramineae, especially in *Zea Mays* (Indian corn), multinuclear cells of the most pronounced character are of frequent occurrence.

If a section be made through the stem-region of a young plant so as to pass through the enveloping leaf-sheaths, the parenchyma cells of

the foliar bases will give most instructive illustrations of nuclear multiplication unaccompanied by cell-division. In the younger leaf-bases each cell contains a single nucleus which is a well-defined, generally spherical body that stains very feebly, except the large and conspicuous nucleolus, which is its most striking feature. As successively older sheaths are examined it will be found that the nuclei increase in size, the nucleoli keeping pace with the general growth. Moreover, the clear, granule-free space, the "Hof" of Rosen (12 and 13), which is clearly seen surrounding the nucleolus, becomes broader and more noticeable with advancing age. The next stage is that in which the nuclei have undergone fragmentation, and several, sometimes five or more, nuclei can be seen in one cell. The direct method is, however, not the only way in which the plurality of nuclei originate within these cells, for I have observed quite a large number of cases in which the multinuclear condition was either partly or entirely due to karyokinetic division. The mother-nucleus of the young cell may undergo karyokinetic division, and by that means give rise to several nuclei in one cell, whilst the neighbouring cells may attain the same end by nuclear fragmentation.

Again, within one cell which contains a number of nuclei one nucleus may show karyokinetic figures, whilst another is as clearly fragmenting. Moreover, out of a group of nuclei, which I have reason to think originated by direct division, some may proceed to divide further by karyokinesis. These cases are of considerable interest as they plainly show that the nuclei, which are in a condition for fragmentation, have by no means necessarily lost their power for active, karyokinetic division.

In the older leaf-bases of *Zea Mays direct* nuclear division of a somewhat different order seems to prevail exclusively.

In the fragmentation of both the younger and the older nuclei the same impulse to divide seems to underlie the process, but the manner in which it acts differs in the two cases.

In order to understand this difference, and since the process in the younger cells of *Zea Mays* does not seem to conform in all respects to the usual descriptions of fragmentation, I may perhaps be excused for touching on this subject at greater length. In what follows I will rely chiefly on the observations which I have made on longitudinal sections of the growing-point of the root of *Zea Mays*, since the steps can here be followed with especial clearness. The same observations can (with greater difficulty, however) be made on the nuclei of the leaf-sheaths. Multinuclear cells occur, but more sparingly distributed than in the foliar organs, in the growing point of the root. The resting nucleus is a spherical or oval body which stains feebly except in the large nucleolus. This nucleolus may in some cases attain an enormous size, as for instance in the cells which are the precursors of a vessel, it usually has a perfectly homogeneous appear-

ance, but in the dermatogen cells vacuoles may be detected in its interior, it is immediately surrounded by a more or less broad investment of perfectly clear substance, the "Hof," which is sharply marked off from the granular, peripheral nuclear substance.

The first step in the division of such a nucleus is the division of the nucleolus. Immediately after division the two nucleoli lie in one "Hof." In the next stage that can be found each nucleolus is surrounded by its own "Hof." This clear belt grows more and more pronounced whilst the granular nuclear substance is gradually encroached upon and finally forms only a peripheral investment to the clear balls of substance which surround the nucleoli. The thin layer of granular substance that lies between the two clear areas appears to disintegrate or at any rate to separate *without any previous constriction*, and the two spheres of clear substance, each containing a nucleolus, are separated from one another and form the daughter nuclei. The clear body-substance of these daughter nuclei becomes later more granular, and the nuclei may move some distance apart.

Observations show that it is not merely a question of the disintegration of the granular layer between the clear areas, but that there is an actual strain pulling the nuclei in two.

It should here be mentioned that every nucleus which is provided with several nucleoli must not necessarily be regarded as indicating a stage of nuclear fragmentation, although by far the majority of resting nuclei have, in *Zea*, only a single nucleolus each.

When older leaf-sheaths of *Zea Mays* are examined, nuclear fragmentations are seen, which differ considerably from those which have been mentioned above, and conform much more nearly to the stages which have been described in other plants by previous observers.

The first and most obvious difference between direct nuclear division in the younger and older cells of *Zea* is that whilst the former takes place without the appearance of constrictions and changes of form in the nucleus, the latter is conspicuously marked by the grotesque intermediate shapes which that body assumes.

The entire absence of constricting nuclei in the young leaf-bases, which are obviously developing multinuclear cells, is the most characteristic and at first the most puzzling feature about them. Another point in which the older nuclei differ from the younger is that in the former no constant relation between fragmentation and a preceding nucleolar division can be made out. Sometimes two or more nucleoli appear here also, and one goes to each fragmentation product, but just as often one nucleolus alone is present throughout the process.

These differences, which at a first glance are so striking, are possibly associated with the alteration in constitution which the nucleus suffers with advancing age. The young nucleus is large, and has every appearance of being rich in water; the nucleus of the older

cells is a smaller, shrunken, and more solid body, provided with a comparatively small nucleolus. One can well understand how those internal differentiations, which we have described for the young nucleus, could not readily take place in the dense body of the more aged nucleus. In consequence there would be no line of weakness formed which would sharply and cleanly break across under the influence of the tension impelling division ; instead, the dense nuclear substance would be drawn out and variously contorted at the line of division.

The nuclear differentiation, accompanied by a constriction which is so seldom found, marks the link between the fragmentation in the younger and older cells. In this case the nuclear substance, whilst not being too dense to allow the internal changes to take place, has yet become, even in the granular substance, too firm for a clean break to be formed between the daughter nuclei.

Judging from these observations on *Zea Mays*, the vexed question whether nucleolar division does or does not always precede nuclear fragmentation, is not one to be answered in a sentence. The age, the general density of the nucleus in that particular tissue or plant, the intensity of the impulse to divide, all have to be carefully considered. The discordant statements made on this subject by eminently careful observers are to be explained by their having examined the same tissues at different ages or grown under different conditions (cf. 6, 11, and 17).

Before leaving the subject it should be mentioned that these older nuclei which are fragmenting by constriction never show karyokinetic figures, and have apparently lost the power of dividing indirectly.

As has been mentioned, the above observations were made on the leaf-sheaths and root-apices of *Zea Mays*. In the latter the directly and indirectly dividing nuclei could be found in adjoining cells, as was the case in the leaf-sheaths. Multinuclear cells of a similar appearance have also been observed in the stem of *Zea*, in the leaf-sheaths of *Secale cereale* (rye), the leaf-sheaths and young stems of *Triticum vulgare* (wheat), the leaf-sheaths of *Hordeum sativum* (barley), and *Dactylis glomerata* (cock's-foot grass).

In conclusion, I should mention that the best results were obtained from young seedlings of the plants mentioned ; plants of *Zea*, a little over a foot high, made excellent material.

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EXPLANATION OF PLATES I. AND II.

Figs. 1-8.—Parenchyma cells from leaf-sheaths of *Zea Mays*. Fig. 2 in longitudinal section; the rest transverse.

Figs. 9-13.—Cells from the growing point of the root of *Zea Mays*. Longitudinal section.

Fig. 14.—Parenchyma cells from leaf-sheaths of *Secale cereale*. Transverse section.

Fig. 15.—*a*. Parenchyma cell from leaf-sheath of *Triticum vulgare*.
b. Parenchyma cells from stem of same. Transverse section.

FRESH FACTS.

Memory in Fishes. L. Edinger. "Haben die Fische ein Gedächtnis?" Das Ergebniss einer Sammelforschung mitgetheilt in der neurologischen Sektion der Versammlung Deutscher Naturforscher und Aerzte in München, 1899. Sonderabdruck aus der Beilage zur *Allgemeinen Zeitung*, Nos. 241 and 242, vom 21 und 25 Oktober 1899. München, 30 pp. We have been favoured with a copy of this interesting paper, embodying the results of observations which have been communicated to Prof. Edinger since he made his appeal for information a couple of years ago. It seems to be clear (1) that the instinctive impulse to flee from certain impressions may be lessened as the fishes find the stimulus harmless and become accustomed to it, and, contrariwise, that they may become shy; (2) that the optic or chemical stimulus normally associated with food may be replaced by the image of the feeder. There is therefore a kind of memory, but it is very different from that of mammals. It may be recalled that Prof. M'Intosh, of St. Andrews, was one of those who answered the question, "Have fishes a memory?" with a decided affirmative, when the matter was discussed a couple of years ago.

More Traces of Neomylodon. Erland Nordenskiöld. "Neue Untersuchungen über Neomylodon listai," *Zool. Anzeig.* xxii. 1899, pp. 335-336. The author has made laborious excavations in the cave at Ultima Esperanza (South Patagonia), where previous digging discovered the pieces of skin, etc., belonging to the somewhat shadowy creature, *Neomylodon listai.* He has found a number of bones which he thinks should go with the skin. Their description will be awaited with interest.

Concerning an Ancient Fish. A. Smith Woodward. "Note on Scapanorhynchus, a Cretaceous Shark apparently surviving in Japanese Seas," *Ann. Nat. Hist.* iii. 1899, pp. 487-489. A shark in all essential respects identical with the supposed extinct genus *Scapanorhynchus* has been obtained from the deep sea off Yokohama, and described by Profs. D. S. Jordan and Mitsukuri. It has been called *Mitsukurina,* but Mr. Smith Woodward points out that in all the generic characters which can be compared it agrees with the above-named predaceous shark of the Cretaceous Seas.

Rôle of the Nucleus. Jacques Loeb. "Warum ist die Regeneration kernloser Protoplasmastücke unmöglich oder erschwert?" *Arch. Entwickelungsmechanik,* viii. 1899, pp. 689-693. In this paper, which the author has been kind enough to send us, there is a fresh suggestion rather than a fresh fact. The suggestion is that the nucleus is the oxidation-organ of the living substance, and that non-nucleated fragments of cells are incapable of regeneration because the oxidation-function has sunk below the required minimum. The fragments gradually die of asphyxia. This should be compared with the observations of Schenk referred to in another part of this number of *Natural Science.*

A Contribution to Experimental Embryology. Jacques Loeb. "Ueber den Einfluss von Alkalien und Sauren auf die embryonale Entwickelung und das Wachsthum," *Arch. Entwickelungsmechanik,* viii. 1899, pp. 631-641, 1 pl. Experiments on the developing larvae of the sea-urchin *Arbacia* show that even extremely minute additions of sodium hydrate solution to the sea-water hasten the development and growth, while acids have the reverse effect. The reason suggested is that weak alkalies promote the oxidation-processes and therefore the synthetic processes in the living substance.

A Triassic Cuttlefish. K. Picard. "Ueber Cephalopoden aus dem unteren Muschelkalk bei Sondershausen" (*Zeitschr. deutsch. geol. Ges.* li. pp. 299-309, pl. xvi. Oct. 1899) describes *Campylosepia triasica*, n.g. et sp. on

the evidence of a curved rostrum and an impression of the pro-ostracum; a few of the septal lamellae are preserved. The author regards it as belonging to the Sepiadae and as a link between the belemnites and cuttlefish, and he compares it with *Belosepia*. The latter, however, is a Tertiary form, and no true *Sepia* is known from Mesozoic rocks. This fact, while increasing the interest of the discovery, leads us to ask for more evidence.

AUTOGAMY IN PRIMULACEAE. A Field Naturalist, M.A. Camb. "The Primrose and Darwinism," *London Quarterly Review*, clxxxiv. October 1899, pp. 209-235. This very interesting and circumstantial indictment of Darwin's conclusions in regard to cross-fertilisation in primroses occurs in a place where it may be overlooked by many botanists. After relating his observations and stating his criticisms, the unknown author says: "It is not possible from the above considerations in reference to the method of Darwin's experiments, and especially also from the above case of the primrose, to avoid the conclusion that Darwin has not established his theory that cross-fertilisation is necessary to the full fertility of flowers. On the contrary, we are of opinion that the primrose gives strong confirmatory evidence to Axell's view, that under *natural* and equal conditions self-fertilisation of flowers is both the *legitimate* fertilisation and the most productive."

ELIMINATION IN SPARROWS. HERMON C. BUMPUS. "The elimination of the unfit as illustrated by the introduced sparrow, *Passer domesticus*," a fourth contribution to the study of variation. Eleventh Lecture in Biol. Lectures at Wood's Holl in 1898. Boston, 1899, pp. 209-226. After a severe storm a number of English sparrows were brought to the anatomical laboratory of Brown University. Seventy-two revived; sixty-four perished; and the author has made a careful comparison of the eliminated and the surviving. He has reached three conclusions:—(1) That the birds which perished were eliminated because of deficiency in certain structural characters possessed by the survivors; (2) the process of selective elimination is most severe with extremely variable individuals, no matter in what direction the variations may occur; (3) disregard of structural qualifications finally produces a throng of degenerates, whose destruction will follow the arrival of adversity.

ANOTHER ENIGMA. RICHARD HEYMONS. "Ueber bläschenförmige Organe bei den Gespenstheuschrecken. Ein Beitrag zur Kenntniss der Eingeweidenervensystems bei den Insecten," *Sitzber. Preuss. Akad. Berlin*, 1899, pp. 563-575, 2 figs. In the head of a European stick-insect, *Bacillus rossii*, there lie near the gullet, and associated with the pharyngeal ganglion, two little vesicles of ectodermic origin which are very puzzling. They are neither ganglionic nor glandular, and contain a central chitinous spherule surrounded by several concentric chitinous lamellae. Perhaps they are comparable to the "corpora allata" which occur in a number of other insects, but they are not the same in detail, and besides we do not know what the "corpora allata" are. Heymons tried by experiment to find out something about their function, but the result was inconclusive. He leaves their nature an enigma, except that he suggests that they may have something to do with the visceral nervous system.

NEW PELAGIC NEMERTEAN. W. McM. WOODWORTH. "Preliminary account of Planktonemertes agassizii, a new pelagic Nemertean," *Bull. Mus. Comp. Zool. Harvard*, xxxv. 1899, pp. 1-4, 1 pl. This new form, like the only other known genus, the "Challenger" *Pelagonemertes*, was taken in the Pacific Ocean from considerable depths. In its leaf-like body, hyaline structure, rhynchocoelom as long as the body, unarmed proboscis, dendrocoelous gut, and absence of cephalic grooves or organs of special sense, it resembles *Pelagonemertes;* but its distinctive features are: a common external opening for mouth and proboscis, supraoesophageal ganglia smaller than the suboesophageal, the presence of a median dorsal vessel, and the large number of lateral diverticula of the intestine.

SOME NEW BOOKS.

THE SPRINGS OF CONDUCT.

The Origin and Growth of the Moral Instinct. By ALEXANDER SUTHERLAND, M.A. In two vols. Pp. xiii. + 461, vi. + 336. London: Longmans, Green, and Co. 1898.

We need make no apology for reviewing this interesting work in *Natural Science*, for, as the author tells us, full half of the book is a detailed expansion of the fourth and fifth chapters of the "Descent of Man." "Darwin showed in these chapters a noble gift of insight, but to have made good his position from point to point, to have left nothing behind him unreduced, would have demanded a labour which neither his own health nor the length of an ordinary life would have permitted." Mr. Sutherland has done good service in filling in Darwin's scheme. Many persons have made *vorläufige Mittheilungen* on the same subject, but Mr. Sutherland has written a treatise of great value. If he had submitted his two volumes to a candid friend at a distance, who rejoiced in the exercise of the blue pencil, if he had made the two volumes into one, if he had avoided such question-begging phrases as "the moral instinct," if he had called his book "The Evolution of Sympathy," he would have commanded an interested audience whom this treatise will never touch. We would not seem ungrateful, the book is the outcome of eleven years of hard work, it is full of careful erudition, it is most intelligibly written; our regret is simply that a lack of worldly wisdom or self-criticism has robbed the book of much of its utility by leaving it so large.

Nowhere else that we know of can we find such a carefully selected treasury of facts bearing on parental care, conjugal affection, and the feeling of kinship—all working towards a theory of the evolution of sympathy on which the author believes morality to be founded. He shows, to our thinking conclusively, how there was worked out among animals an inheritance of altruistic emotions which became in man the springs of good conduct; but we do not think that he has been equally successful in showing how man became moral, that is, became accustomed to "think the ought," to control his conduct in reference to general ideas and ideals. According to Mr. Sutherland, "the moral instinct is in social animals the result of that selective process among the emotions which tends to encourage those that are mutually helpful, and to weaken those that are mutually harmful," but what he has actually been working at is the growth of sympathetic emotions, not the origin of the distinctively ethical note which characterises many human actions. The evolution of altruistic feelings is one thing; the distinction between good behaviour and moral conduct is another; and the book seems to us to fail seriously in not appreciating the distinction. But as a treatise on the evolution of sympathy, on the springs of good conduct, it is admirable, and most useful to biologist and moralist alike.

X.

THE TIDES.

The Tides Simply Explained, with Practical Hints to Mariners. By the Rev. J. H. S. Moxly, B.A., T.C.D. Pp. v. + 151. Rivingtons: London, 1899.

There are many excellent points about this little book, although we are not prepared to accept all the positions taken up by the author. Laplace, Airy, Kelvin, and G. H. Darwin, our recognised exponents of tidal theory, are severely dealt with, especially the last named. The author's object is to show that the much-reviled "Equilibrium Theory"—the theory, in fact, which is usually described in our elementary physical geographies—is, in a slightly modified form, amply sufficient to explain all tidal mysteries. Consequently he argues strongly against the rival kinetic or perturbation theory as it might be termed. In his criticism of Darwin's dictum that a vertical force cannot produce sideways motion, he seems, however, to confuse vertical force with vertical pressure; and there is a good deal of loose and inaccurate reasoning between pp. 38 and 40—reasoning which fortunately does not affect the main design of the book. In the constructive part of his book Mr. Moxly is decidedly at his best, and we are not acquainted with any clearer statement of the results that (with the assumption of the two tides each day) naturally follow from the equilibrium theory. In some of his discussions he is particularly happy, as, for example, in his account of the single daily tide in high latitudes, of the high tides in the Bay of Fundy, and other anomalies. C. G. K.

A TREATISE ON CRYSTALLOGRAPHY.

Crystallography. By W. J. Lewis, M.A., Professor of Mineralogy in the University of Cambridge. Cambridge Natural Science Manuals, Geological Series. Pp. xii. + 612, 553 figs. The University Press, 1899. Price 14s. net.

This substantial volume is a very full and complete geometrical treatise, covering much the same ground as Professor Maskelyne's "Morphology of Crystals," which was published by the Oxford University Press in 1895. The aim of the author is evidently to supply the practical needs of university students, to whom the drawing and calculation of crystals measured in the laboratory is an exercise by which a knowledge of Crystallography can best be obtained; two chapters of considerable length devoted to the subject of crystal drawings and projections are accordingly introduced at an early stage, and the general description of the various classes of crystals, which occupies the greater portion of the book, is illustrated by abundant examples of drawings and calculations very fully worked out. Frequent references to crystals in the University Collection indicate that the book contains many original observations.

The chapters on the systems and their various subdivisions are followed by a chapter on twin crystals, which occupies about 100 pages, and is probably the most extensive general treatment of this subject which has been published since Sadebeck's volume on "Applied Crystallography."

The book presents the appearance of a mathematical treatise, and may discourage the non-mathematical student, but the methods of proof employed will be found to be in reality simple and so expressed as to demand no advanced mathematical knowledge; they are consequently somewhat laboured and lengthy, but the more concise and elegant treatment by analytical methods is given in a chapter at the end of the volume.

A short chapter on the physical properties of crystals is introduced merely

for the purpose of justifying the classification into seven systems; frequent reference to the optical characters is also made in the detailed description of various crystals.

Several new terms are employed; most of the classes formerly known as hemimorphic are called acleistous; monoclinic crystals are divided into a gonoid, a plinthoid, and a hemimorphic class; an axis of symmetry which is polar is called uniterminal, a name that appears awkward where all the other terms are of Greek origin.

The representation of crystal axes by interrupted dots and dashes is of doubtful expediency, since they become difficult to distinguish among other lines; but the author ingeniously makes use of this contrivance to indicate their "order" when they are axes of symmetry by the numbers of dots. The number of the chapter might have been given at the head of each page, for frequent references are made to previous chapters.

The reader will perhaps not expect to find a philosophic treatment of the principles which underlie the geometry of crystals in a book designed for the practical instruction of students; if the subject be new to him he may wonder why a crystal is treated merely as an isolated problem in drawing, projection, and calculation; but let him master the contents and we think that he will acquire a very considerable knowledge of geometrical crystallography, which will set him thinking about the signification of the beautiful laws which prevail in this subject.

CLASSES OF CRYSTALS.

Darstellung der 32 möglichen Krystallklassen. By H. BAUMHAUER. Leipzig: Engelmann, 1899; 36 pp., 32 figures, and 1 plate.

Much attention has recently been paid to the subject of crystal symmetry, and the treatment and nomenclature of the 32 classes have undergone many changes.

In this short pamphlet Professor Baumhauer adds another to several attempts which have been made to describe these classes in a simple and systematic manner. It will be sufficient to say here that he classifies them according to their axes of symmetry, and distinguishes between those which are and those which are not intersections of symmetry planes by the not very happy terms homogeneous and inhomogeneous; axes perpendicular to a symmetry plane are distinguished by the equally unsatisfactory term symmetrical.

The plate which gives a summary of the classes succeeds perhaps better than any previously published, in making the symmetry apparent, and will be of use to teachers.

ROCK-ANALYSIS.

Praktische Anleitung zur Analyse der Silicatgesteine. Pp. 86. Leipzig: W. Engelmann, 1899.

This, as its title further sets forth, is a translation by Dr. E. Zschimmer of the introductory portion of Bulletin No. 148 of the United States Geological Survey, the concluding part of that work being a most valuable series of rock analyses. These analyses are omitted in the translation, which deals only with the analytical methods adopted by Prof. F. W. Clarke and Dr. W. F. Hillebrand in the laboratory of the United States Geological Survey. The methods aim at an exactitude in rock analyses hitherto found wanting in the published work of many of the earlier chemists who have contributed to our knowledge of the composition of rocks. In the analyses given in the Bulletin just cited, extremely small quantities of elements have been detected and estimated in

rocks in which their presence would hardly have been suspected, and which in ordinary analyses would have been unsought and overlooked. The translation contains some additional remarks upon and references to the later observations of Dr. Hillebrand, and on p. 33, a woodcut, not given in the original Bulletin, illustrates the construction of a modified form of Gooch's apparatus, as employed in the U.S. Survey laboratory for the determination of combined water. The translation is well printed and has an index, but although it is a most useful and convenient publication, the original Bulletin No. 148, with its well-tabulated analyses, will probably be more frequently consulted in this country than Dr. Zschimmer's careful translation.

MESOZOA AND SPONGES.

Traité de Zoologie Concrète. Leçons professées à la Sorbonne. Tome ii. 1re Partie. Mésozoaires, Spongiaires. By YVES DELAGE and EDGARD HÉROUARD. Pp. ix. + 244, with 15 coloured pls. and 274 figures in the text. Paris: Librairie C. Reinwald, Schleicher Frères, 1899. Price 12s. 6d.

The new instalment of this great work sustains the high level of its predecessors in its fulness and clearness of exposition, and in its liberality of excellent illustrations. The first part contains the fullest connected account as yet published by the so-called Mesozoa, and is therefore of great interest. Four classes are recognised:—(1) Mesocoelia, viz. *Salinella*; (2) Mesenchymia, including *Treptoplax* and *Trichoplax*; (3) Mesogonia, comprising Dicyemiae and Orthonectiae; and (4) Mesogastria, viz. *Pemmatodiscus*. An appendix treats of *Physemaria*, *Cementaria*, *Pompholyxia*, *Kunstleria*, and *Siedleckia*. The authors have conferred a great boon on zoology, in bringing together the available information in regard to these obscure creatures which are as interesting as they are puzzling.

The second part deals with the sponges, to our knowledge of which Prof. Delage has made some notable contributions. As was expected, there is a careful discussion of the affinities of the class, in which Delage's own views are naturally followed, though the diversity of opinion is duly recognised. The classification adopted is as follows:—I. Calcaria, including Homocoelida and Heterocoelida; II. Incalcaria, including Triaxonia (Hexactinellida and Hexaceratida) and Demospongiae (Tetractinellida, Monaxonida, and Monoceratida). An appendix deals with the doubtful Abyssospongea, which probably do not deserve the name. A zoological treatise on different lines may well be conceived, but it will be hard to excel this one in clearness and fulness, or in beauty of illustration.

ELEMENTS OF VERTEBRATE EMBRYOLOGY.

Die Elemente der Entwickelungslehre des Menschen und der Wirbelthiere. Anleitung und Repetitorium für Studierende und Aerzte. By Dr. OSCAR HERTWIG, Director of the Anatomical-Biological Institute of the University of Berlin. 8vo, pp. vi. + 406, with 332 figs. Jena: Gustav Fischer, 1899 [dated 1900]. Price 7.50 marks, 8.50 bound.

In twelve years Prof. Hertwig's well-known *Lehrbuch* has passed through six editions, and has been translated into English, French, Italian, and Russian; and no one who has used it, whether as student or teacher, will wonder at its great success. It is a model of lucidity, it is well illustrated, it is flavoured with the salt of general ideas, and it is full of suggestion.

But as he worked at the later editions, Prof. Hertwig began to feel that it was impossible to cater for two sets of appetite. The expert wished for more

detail and the student for less. Hence the publication of the present "Elements" which is adapted to the busy student, while further editions of the *Lehrbuch* will be specialised for his teachers. Beyond that some paragraphs have incorporated recent results, there is little to distinguish this new volume from its predecessors, but it is shorter and perhaps simpler, and more emphasis is given to the summaries. All must wish it good speed.

CO-OPERATIVE GEOGRAPHY.

The International Geography. By SEVENTY AUTHORS. Edited by HUGH ROBERT MILL, D.Sc., etc. Pp. xix. + 1088. London: George Newnes, Limited, 1899. Price 16s.

The method of compiling a hand-book of geography by the collaboration of a number of authors, each of whom is a native of the country he describes, or has had especial opportunities of making himself thoroughly conversant with the subject of which he treats, has self-evident advantages. It has, however, its drawbacks. Authors are apt to give undue prominence to their particular theories, and to entertain different views as to what details should be included, with the consequence that the book is lacking in uniformity. For the contributors to this work the editor has drawn up a set of rules, setting forth the heads of information and the order in which they should be discussed. But these rules have not always been followed. In some cases the geology has not been touched upon, in others internal communications have received little attention, and boundaries are sometimes clearly defined where they are definitely fixed and marked on maps, while no mention is made of others which are still subjects of dispute.

Two or three instances will suffice to show how the division of labour has resulted in a want of unity in the whole. On p. 16 it is stated that Thales invented gnomonic projection, and the reader will naturally turn—in vain—to the preceding chapter to find out what that projection is. The writer of the chapter on Mathematical Geography has evidently had some difficulty in compressing all he had to say into the space allotted, and could not foresee that his colleague would mention a projection now used only on charts for Great Circle sailing. The apparent antagonism contained in the sentences, "Further north the Parana takes the name of Paraguay" (p. 850), and "They (the Parana and Paraguay) both rise in Brazil," needs only a few words of explanation. Another case is of more importance, the two sentences being contradictory; on p. 423 we are told that "no definite geomorphic line divides them (the islands of the archipelago between Asia and Australia) into an Asiatic and an Australian group. 'Wallace's Line' . . . is only a faunal boundary"; and on p. 533 we find, "This line, therefore, clearly follows what, in very recent geological times, was the shore of the continent of Asia"—a boundary still marked by a belt of deep water.

Other small discrepancies might be pointed out, and are to be expected in so comprehensive a work on its first appearance, and considering the vast amount of labour involved in gathering together so large a staff of collaborators, providing for the translation of articles written in foreign languages, and in the general supervision of the whole work.

Nevertheless, a large measure of success has been attained, the individual chapters are on the whole of a high order, many of them being really excellent, more particularly those from the pens of professed geographers, who are accustomed to regard a country from all points of view, and treat the physical features, geology and climate, in connection with the occupations of its inhabitants. Professor W. M. Davis' description of the United States deserves especial mention, and the editor's chapter on the British Isles has also great merit, apart from one or two slips in the historical paragraphs.

When a new edition is required, as no doubt it will be, some alterations should be made in the allotment of space, on the basis of the importance of the subject, and not on a comparison of text-books; for "The International Geography" is more than a mere text-book. It may be found advisable to slightly extend the work, so as to form two volumes of more moderate size. Space might also be profitably saved by the omission of the historical paragraphs. A history of a country compressed into a single paragraph is useless, nor has history a *locus standi* in a geographical work unless it be connected with the physical features. Except Professor Davis, who indicates briefly how the natural features determined the lines of penetration into the American continent, hardly any author gives more than a few bare historical facts.

In conclusion, we must give a word of praise to the numerous small sketch-maps and diagrams scattered throughout the volume. They are clearly and carefully drawn, and exhibit the particular facts or phenomena they are designed to impress on the reader, unobscured by unnecessary details.

PALAEONTOLOGY IN MINIATURE.

Paläontologie. By Dr. RUDOLF HOERNES, Professor in the University of Graz. Pp. 212, with 87 figs. Leipzig: G. J. Göschen, 1899. Price 80 pf.

This primer of palaeontology is a little marvel. Of a size familiarised to many students of ten years ago by Macalister's "Zoology" and MacNab's "Botany," with excellent type, with over fourscore excellent figures, and with over 200 pages of sound, descriptive palaeontology by a well-known authority, it costs about nine pence! We should like to hear the comments of a British publisher on this the 95th number of the Sammlung Göschen. After an introduction of 32 pages on the scope and aims of palaeontology, there are 50 pages on the plants of the past, and more than 100 on the animals. The author has been more conservative than was necessary, thus Spongiae are ranked under Coelenterata, Anthozoa precede Hydrozoa, Vermes are separated by Echinodermata from Bryozoa and Brachiopods, and so on. As we peruse it, however, some of the simplicity of the table of contents disappears, for while Pisces form the first class of Vertebrata, it is expressly noted that forms like *Amphioxus* and *Palaeospondylus* may well be referred to special classes. We have great admiration for this little book, but would make two general criticisms:—(1) that there is too little suggestion of history, of movement, of progress, of evolution, in short, of the keynote of a true palaeontology; and (2) that in a primer, above all, insecure conclusions should be very cautiously stated, and surely the doctrine of Kinetogenesis, for instance, which finds a prominent place in the introduction, is still an insecure generalisation.

Missouri Botanical Garden Tenth Annual Report. St. Louis, Mo., U.S.A., published by the Board of Trustees, 1899.

This well-known publication sustains its former high standard in every respect. The present report is specially valuable, because it gives a résumé of the work of the Garden for the first decade of its existence, 1889-98; and also a complete index, of 51 pages, of the ten volumes of the Record.

The objects of the late Mr. Shaw, the founder, are summarised, and not the least important of these is the encouragement of botanical research in the broadest sense. The endowment of the generous founder enables these objects, which he contemplated, to be carried out successfully, and permits of the gradual growth and extension of the Garden in all its branches.

The interchange of seeds, cuttings, and small plants with similar institutions

abroad, is another noteworthy feature of the Garden. The herbarium grows apace, and plans have been prepared for the construction of a museum. The library alone ought to attract students from a wide area; it has 32,000 books, and over 200,000 index cards; but it is specially rich in pre-Linnean works, over 500 of which were gifted in 1892 by the late Dr. Ed. Lewis Sturtevant.

An able biographical sketch of Dr. Sturtevant by Prof. C. S. Plumb is given in the Record. Sturtevant did much for the cause of agriculture in America, and his name will long be associated with the famous herd of Ayrshire cattle which he established, and with the cultivation of maize.

A short illustrated paper on "A Sclerotoid Disease of Beech Roots," by Hermann von Schenk, appears as research work from the Garden. The condition was found on one clump of trees, and their roots were devoid of the fungus-covering or mycorhiza common to beeches and most other forest trees. Further investigation may throw light on this unusual condition.

The most important part of the Report consists of a paper by F. Lamson-Scribner on "Notes on the Grasses in the Bernhardi Herbarium, collected by Thaddeus Haenke, and described by J. S. Presl." This article consists of 25 pages of letterpress and 54 full-page plates, which will be admired by all students of the Gramineae; the drawings are by Mrs. M. D. B. Willis, *née* Baker, and they recall to British botanists the faithful work of Parnell, but in the matter of reproduction, on paper of superior finish, the former surpass the latter. The collection includes a large number of American species from the Pacific coasts of Mexico, California, and Peru; many from the Philippines, and a few from Nootka Sound. Most of the genera, and certainly the facies of these grasses, resemble those of South Africa. One word of caution may be permitted; the terminology of Presl has been adhered to, but the majority of the genera have been revised since 1830, and if the Herbarium of the Missouri Botanical Garden is to be of use to modern students, its classification must also be modern.

R. T.

The North American Slime-Moulds, being a list of all Species of Myxomycetes hitherto described from North America, including Central America. By T. H. MACBRIDE, A.M., Ph.D., Professor of Botany in the State University of Iowa. Pp. 231 with 18 plates. New York and London: The Macmillan Co., 1899. Price 10s. net.

This book is likely to form the classic on the subject for North America. Apart from the scientific interest of the work one is compelled to admire the binding, the paper, the type, and the beautiful photo-reproductions of the plates.

The only general works published within recent years on the subject are Massee's "Myxogastres," in 1893; Lister's "Mycetozoa," in 1895: a worthy third is Professor Macbride's "Slime-Moulds," if we may follow the author in giving them this name, for it is unfortunate that botanists cannot agree as to the proper name that should be applied to those organisms which seem to occupy the border line between the two organic kingdoms.

The volume begins with a general description of the vegetation and reproduction of the slime-moulds. It is pointed out that they are not unicellular organisms, as was formerly taught, but multinuclear and karyokinetic. From the resemblance of the protoplasmic mass to that of a giant amoeba arises the claim of the zoologist to consider the slime-mould his special property.

But the author prefers to leave the question of their higher relations alone, recognising that no one test can be applied as a universal touchstone to separate plants from animals. As a matter of fact the study of the slime-moulds rests chiefly with the botanists, and it is expedient to leave it in their hands.

Over 400 species of slime-moulds have been described, and half of these

have been recognised in the United States. One degenerate species, *Plasmodiophora brassicae*, occasions the disease known as "club-root" in cabbage, and "finger-and-toe" in turnip; while it is alleged that *Plasmodium malariae* may be the cause of malarial fever, and if it turn out to be a slime-mould, then the group suddenly acquires an unusual human interest. Apart from these two the slime-moulds are of no economic importance. Advice is given regarding the collecting and preserving of material, but the greater part of the book is taken up with classification. The descriptions of the genera and species are most carefully given, while the measurements of the spores are in microns.

For the sake of the British farmer we wish that the following statement made by Professor Macbride regarding *Plasmodiophora* were true for the British Isles:—

"Careful search continued through several years has not availed to bring this species to my personal acquaintance." It is unfortunately too true, however, that British farmers lose thousands of pounds annually through the ravages of *Plasmodiophora* in their turnip crops. R. T.

A CRITICISM OF THE BIOLOGICAL GOSPEL.

From Comte to Benjamin Kidd. The Appeal to Biology or Evolution for Human Guidance. By ROBERT MACKINTOSH, M.A., B.D., D.D., Professor at Lancashire Independent College. xxii. + 287 pp. London: Macmillan and Co., 1899.

"The appeal to biology, outlined by Comte, newly defined and emphasised by Darwinism, has now been stated in the most extreme form logically possible," by Mr. Benjamin Kidd. Dr. Mackintosh has weighed the results of this appeal in the balances and finds them very short weight. In fact, he indicates that the appeal is gratuitous. There is available elsewhere much better guidance for human conduct than biology can offer, and the appeal to biology is apt to be misleading, as well as unsatisfactory. These are hard words, but it must be remembered that biology is still very young, much too young to give advice. Some have tried to force its hand and the results do not look well, but it was not a fair game to play. The science is too young to become a basis for the art of life.

The author is brilliantly clever; there is not a dull page in the book, perhaps not a dull sentence; his criticisms of even purely biological matters make one feel what the science has lost in his being outside of it. To contradict him is impossible, for he is so reasonable; to correct him is impossible, for the time is not yet ripe; to believe him is (for a biologist) impossible, for he proves too much. It seems to us that biology, preoccupied with its own concrete problems, has simply stammered like a child when forced to confront the big problem of human life; it has something to say, but it is not ready to say it. That it will eventually have a rational word to say, and one which will rhyme with the best word of the moralist, we never doubt.

Part I. deals with Comtism, the appeal to biology, the appeal to history, and the doctrine of altruism. Part II. discusses the "simple evolutionism" of Spencer and Leslie Stephen. Part III. deals with Darwinism, or Struggle for Existence, and includes a splendid chapter on the metaphysics of natural selection. Part IV. has to do with Weismann and Benjamin Kidd, so widely apart, and yet in one respect so near akin. Finally we have a summary and conclusions. It is easy to write these lines; but to criticise is another matter, and we frankly confess that we must refrain, though the temptation is great. We refrain for this reason, that although we are unable to agree with the author's central conclusions, we feel that he has done great service in showing that the appeal to biology is premature. X.

OUR PLAY.

Die Spiele der Menschen. By KARL GROOS, Professor of Philosophy in Basel. Pp. 538. Jena: Gustav Fischer, 1899. Price 10 marks.

Two or three years ago Professor Groos rather startled us by his book "Die Spiele der Thiere," in which he showed that play was one of the most serious things in the world. This book was translated last year by Mrs. Baldwin, and published, with a preface and an appendix by Professor J. Mark Baldwin, under the title "The Play of Animals: a Study of Animal Life and Instinct." The author's thesis has thus become familiar. Play is not mere by-play, but a matter of serious moment; it is the expression of an instinct developed by natural selection, and justified (1) because the playful young animal can rehearse without responsibilities, and practise for its future life without serious consequences, play being really the young form of work; and (2) because the young animal is able in play to learn many lessons which would otherwise have to be inherited as special instincts, thus lessening the burden of inheritance, and putting a premium on intelligence. To which may be added that the play-period affords elbow-room for new departures—an "*Abänderungsspielraum*"—before natural selection begins to operate with its usual sternness.

In the volume now before us Professor Groos applies his "practice theory" of play to the games of children and men, and on the whole seems to succeed in corroborating it, though the case does not seem to us quite so clear as it was when animals alone were dealt with. The first section deals with playful experimenting—sensory, motor, intellectual, and emotional. The second section discusses combative play, love play, imitative play, and social play. Then follows a general consideration of the theory of play, looked at from six points of view—physiological, biological, psychological, aesthetic, sociological, and educational.

We do not know whether to admire most the author's erudition, or his vivacity, or his intellectual perspective. The result is certainly a notable contribution to the theory and art of life. It invests the familiar adage, "All work and no play makes Jack a dull boy," with a profoundness of solemn meaning.

It is to be hoped that this volume will also be translated by Mrs. Baldwin, who dealt so successfully with the first, for it is a book that ought to have the widest possible circulation, not merely because it is a thorough vindication of what we may call the Darwinian theory of play, but also for its practical suggestiveness to parent and teacher, physician and artist.

MATSCHIE'S CATALOGUE OF FRUIT-BATS.

Die Fledermäuse der Berliner Museums für Naturkunde: 1 Lieferung, Die Megachiroptera. By P. MATSCHIE. 8vo, pp. viii. + 103, pls. 14. Berlin: George Reimer, 1899. Price 24 marks.

The British Museum "Catalogue of Chiroptera," by the late Dr. Dobson, having been published so far back as 1878, has long been completely out of date; and naturalists should therefore welcome Dr. Matschie's new descriptive synopsis, of which the first instalment is before us. It appears that the late Professor Carl Peters, Director of the Berlin Museum from 1857 to 1883, contemplated the publication of a monograph of the Bats, for which were prepared no less than 75 lithographic plates, executed by the well-known artists F. Wagner and G. Mützel. These plates remained in the hands of Herr G. Reimer, the publisher, after the death of Professor Peters, but no accompanying MS. was found among the effects of the latter. This being so, Dr. Matschie determined to write the text for a descriptive synopsis of the

order *de novo*, while the publisher undertook to supply such additional plates as were required to bring the work thoroughly up to date. At least 15 of such new plates are announced for issue, 11 of which have been drawn and lithographed by the late Mrs. Matschie. With such a wealth of illustration, the work starts with a strong promise of success. It is announced to be completed in four parts.

The present fasciculus deals with the important and interesting group of Megachiroptera or Fruit-Bats, all the members of which Dr. Matschie follows his predecessors in placing in the single family *Pteropodidae*. In this family the author recognises 20 genera and 122 species, together with numerous sub-genera and sub-species. And here it may be remembered that, although the distinction is clear enough in the systematic index, it would have been better if the number of sub-species had been more markedly distinguished in the text from those of species. Moreover, to our thinking, a few more plates of the animals themselves, in addition to the numerous figures of skulls, would have added decidedly to the general interest of the fasciculus, and have made it more attractive at least to the amateur naturalist.

In regard to the limits of genera the author differs considerably from some English naturalists. He regards, for instance, the curious *Pteralopex atrata*, of the Solomon Islands, as representing merely a sub-genus of *Pteropus*, instead of a genus by itself; while, on the other hand, *Cynopterus marginatus* from Sarawak, described by Mr. O. Thomas in 1893, is considered worthy of separation as a distinct genus (*Dalionycteris*). Moreover, there are several important emendations on the Dobsonian nomenclature, *Rousettus*, Gray, replacing *Xantharpyia*, Gray, while *Gelasinus*, Temminck, stands for the preoccupied *Harpyia*, Illiger. If this latter change can be substantiated it will save the transference of the name *Cephalotes* from the genus it usually stands for to the above-named group (*Harpyia*), as has been proposed by Mr. T. S. Palmer; but it is very doubtful whether experts will admit the innovation. Although changing preoccupied names when they are literally identical with their precursors, Dr. Matschie refuses to admit that a name like *Macroglossa* necessitates the abolition of *Macroglossus*; but here, again, we are on dangerously debatable ground.

So far as we have tested them, the generic and systematic definitions seem clearly and accurately drawn up; but how these work in actual practice can only be demonstrated when new genera or species have to be described. Special value attaches to the author's notes on the distribution of the species of Epauletted Bats (*Epomophorus*) in Africa, and the zoo-geographical sub-regions of that continent, but there seems too much tendency to make the species fit in with the regions.

MULTIPLICATION OF MOSSES.

Untersuchungen uber die Vermehrung der Laubmoose durch Brutorgane und Stecklinge. Von Dr. Carl Correns. 8vo. Pp. xxiv. + 472, with 187 figures. Jena: G. Fischer, 1899. Price 15 marks.

By no means the least exciting group of plants are the Mosses. How interesting their position in the scale of plant-life, so far removed, excepting only their close allies the Liverworts, from everything else, and separated by as great a gulf from the less highly organised Algae as from the more highly organised Ferns! How remarkable their life-history, with its clearly marked division into two phases, distinct but never separated! How puzzling the comparison of organs and members with those of the higher plants! On the one hand the leaf that is not a leaf, on the other hand the unmistakable leaf-like character in structure and function of the base of the highly-organised spore-capsule. But perhaps the most striking feature of the group is their

power of reproducing vegetatively. Almost any portion of the plant will, under favourable conditions of moisture and temperature, give rise to a new individual, and there are also a great variety of means by which this can be naturally effected. It is this last aspect of their biology which forms the subject of Dr. Correns' substantial contribution to Muscology, a work which we are sure will be perused with much interest by the increasing number of botanists who are specially interested in the Mosses. The book, which is partly special, partly general, opens with a short introduction (pp. xvii.–xxiv.), in which terms are explained and methods described. The "brood-organs" are in brief those organs which are definitely produced for the purpose of vegetative reproduction; the "Stecklinge," on the other hand, are those parts of the plant which will on separation form a new individual, but have not been definitely developed to that end. The greater part of the book (pp. 1–322) is "special," comprising first an account of the investigated cases of multiplication by "brood-organs," arranged systematically in tribes, families and genera, and secondly those species in which the other method obtains. This is followed by a "general" part (pp. 325–360), arranged in five sections, treating of the morphology and phylogeny, structure, development and germination, more especially of the "brood-organs," and of their value for systematic purposes. A bibliography occupies a few pages, and the book closes with an index of the plants mentioned in the text. One cannot have too many illustrations in a work of this kind, and the 187 which are distributed through the text form a valuable help to the elucidation of the subject-matter. R.

ANOTHER BOOK ON BACTERIA.

Bacteria, especially as they are related to the Economy of Nature, to Industrial Processes, and to the Public Health. By GEORGE NEWMAN, M.D., F.R.S.E., D.P.H. (Camb.), etc., Demonstrator of Bacteriology to King's College, London. Pp. viii. + 351, with 15 micro-photographs by E. G. Spitta. London: John Murray, 1899. Price 6s.

In his short preface the author expressly disclaims any attempt to write either a record of original work or a laboratory text-book. His object is merely to discuss in a popular scientific form the present state of knowledge concerning bacteria. As the title indicates, the bacteria considered are not only those capable of producing pathogenic effects, but include the vast number of those which are concerned in natural and industrial processes.

Embracing so wide a range of subject, and being designedly written to suit the lay reader, the descriptions are unavoidably often sketchy and incomplete. But the author is in his happiest vein when dealing with the rôle of bacteria in natural processes, and with their industrial application, and to this the greater part of the book is devoted. The chapters on the bacteria in the soil, the bacteria of fermentation, and the bacteria of milk and its products, are specially valuable to medical readers for the lucid and interesting account they give of the far-reaching beneficial effects of bacteria. The ordinary student of medicine is too apt to associate bacteria with disease alone, and the author is to be congratulated on presenting in so attractive a form an outline of the immensely greater activities which these lower vegetable organisms possess. Whether the brief description of the chief pathogenic bacteria could be of equal value or interest to the lay reader, we are inclined to doubt, and it would be easy to criticise adversely some of the details given in this section. But the shortcomings of this latter part are only of minor importance, and do not detract from the value of the preceding chapters.

The book is illustrated by several good illustrations from micro-photographs, and by a number of outline drawings of bacteria, for which the writer claims only a diagrammatic significance. It may be permitted to point out that some

of the latter have scarcely even that claim—notably the drawing of the bacillus of malignant oedema on page 174.

We would, however, cordially recommend the book to all who desire to gain an introduction to the vast science of bacteriology, and, more particularly, to medical men who take any interest in natural processes outside, but intimately related to, their immediate profession. D. A. WELSH.

PRACTICAL CHEMISTRY.

Laboratory Manual—Experiments to illustrate the Elementary Principles of Chemistry. By H. W. HILLYER, Ph.D. New York: The Macmillan Company; London: Macmillan and Company. Pp. vi. + 200 (100 pages blank). Price 4s. net.

The character of this book fully corresponds to its second title, and the teacher of elementary students both in school and college will find it useful in laboratory work. The book is divided into two parts, Part I. dealing with preparation and properties of the elements and their compounds, whilst Part II. is a guide to experiments in verification of quantitative laws. The experiments are, on the whole, very well chosen, and the directions for their performance are definite and accurate, illustrative diagrams being given where necessary. In the last section, dealing with "Molecular weight by chemical means," scarcely sufficient stress is laid on the fact that the basicity of the acids to which the method is applicable must be determined beyond all doubt if a conclusive result is to obtained. Students almost invariably ignore this essential condition, so that the point ought to be specially emphasised.

A FRENCH CONCHOLOGY.

1. Les coquilles marines des côtes de France. By M. Locard. Large 8vo, pp. 384, with 348 figures in the text. Paris: J. B. Baillière et Fils, 1892. Price 18 francs.
2. Les coquilles marines au large des côtes de France. By M. Locard. Large 8vo, pp. 198. Paris: J. B. Baillière, 1899. Price 6 francs.

By the issue of the second of the volumes above mentioned, Mons. Locard has completed the publication of his "Conchyliologie Française." As stated in the introduction to the first volume, his object was to give short but precise descriptions of all the species of shells which are to be found in French waters, so that the student and collector might be able to name his specimens without having recourse to large and expensive works.

The first volume, published in 1892, dealt with the shells of the French coasts. A second, published in 1893, gave descriptions of those living in the fresh and brackish waters of the country. A third, published in 1894, described the terrestrial shells; and now the whole is completed by a volume on the marine shells found outside the French coasts between the coralline zone and a depth of about 2000 metres.

We propose to notice the first and last of these volumes, which contain descriptions of all the genera and species of shells that have been found in the seas around France. Mons. Locard's work is essentially a conchological one. The animals themselves are not described; neither are questions of classification or synonomy touched upon, the reader being referred for these points to his previous work, the "Prodrome de Malacologie Française." But he defines the families and genera which he has adopted, and gives a description of each species of shell, with a mention of its geographical and bathymetrical distribution. Many of the species are illustrated by figures in the text, of which there

are 348 in the first volume, but there are no illustrations in that on deep-water shells.

Most of the figures are either of natural size or are enlargements of small species, but those of larger shells are unequally reduced, some being one-third, some one-half, some two-thirds of actual size, while one (*Tritonium nodiferum*) is only one-eighth of such size, which gives a false impression of its dimensions. The figures are not woodcuts, but are photo-prints from good drawings, and suffice for purposes of recognition.

In the first volume he enumerates 1186 species, including 14 Brachiopods, but there are many among them which other conchologists would probably regard as varieties. The numbers of each class are as follow :—

Gastropoda	777
Scaphopoda	11
Lamellibranchiata . . .	384
Brachiopoda	14
	1186

In his last volume he gives a brief account of the successive dredging expeditions by which the deeper waters have been explored, from that of the Porcupine in 1869 to those undertaken by private individuals in 1895 and 1896. The species obtained from these greater depths number 625, and 286 of them do not occur in the shallower waters. Thus he makes the total number of shell-bearing Mollusca found on or near the coasts of France to be 1488.

M. Locard may be congratulated on having completed a work of so much labour, and one which cannot fail to be useful to all who are interested in the molluscan fauna of the seas around France. Moreover, as a large number of these species occur also on our own coasts his volumes will also be of service to British conchologists.

A. J. J-B.

TOWARDS PERFECTION.

Animal Biology, an Elementary Text-Book. By C. Lloyd Morgan, F.R.S., Professor of Zoology and Geology in University College, Bristol, and Lecturer on Comparative Anatomy in the Bristol Medical School. Third Edition, revised. 8vo, pp. viii. + 313, with 135 figures. London: Longmans, Green, & Co., 1899. Price 8s. 6d.

This well-known and much-appreciated text-book has been modified a little to meet changes in the requirements of the London University examinations, part of it has been at the same time rewritten, and many illustrations have been added,—the result being that the book, so excellent before, has made a marked step towards perfection. It is one of the soundest books that can be put in the student's hands.

At the same time, we have one general criticism to make,—that the book is even in its improved form distinctly smaller than its title. Prof. Lloyd Morgan has in other works made biologists his debtors by his lucid and balanced exposition of the general problems of biology, and by his original contributions towards their solution; he has also elsewhere discoursed in a most interesting way on the habits of birds and beasts, and shown how much may be gained from their study; but of all this there is little trace in the volume before us, which conforms with others in being mainly morphological, differs from most in giving a fair place to physiology, but agrees with almost all in leaving out bionomics. Perhaps the author is right in his reserve, but we doubt it—*for him*. His position, however, is indicated in the sentence,

"With regard to aetiology, the aim will be rather to pave the way for a study of causes by an accurate presentation of facts, than to deal at any length and more than incidentally with the theory of evolution or the doctrine of descent."

J. A. T.

VARIATION-STATISTICS.

Die Methode der Variations-statistik. By Georg Duncker. Pp. 74, with 8 figures. Leipzig: Engelmann, 1899. Price 2 marks, 40 pfg.

As Dr. Duncker explained his position in the last number of *Natural Science*, as Mr. H. M. Kyle discusses the same method in the present number, and as Professor Davenport has published an English guide to the use of the method, we need not waste space by trying to summarise this booklet on the method of variation-statistic. We believe that it is not altogether perfect—it would have been almost a miracle if it had been—but it is a clear statement of the method by one who has used it to good purpose; and we are grateful to Dr. Duncker not only because he has been a pioneer in a fruitful path of investigation, but because he has made it possible for any one with a head on his shoulders to follow in his steps.

NOMENCLATURAL CHANGES IN THE EDENTATA.

Elsewhere we have called attention to certain emendations in the nomenclature of the Chiroptera. A paper by Mr. T. S. Palmer in the *Proc. Biol. Soc., Washington*, vol. xiii. p. 71, suggests others among the Edentata. In an earlier part of the same journal for the current year Mr. G. S. Miller urged that the Armadillos commonly known as *Xenurus* should be designated *Tatoua*, Gray (1865), on account of the preoccupation of the former term. Now Mr. Palmer states that *Tatoua* must itself yield place to the still earlier *Cabassous*, M'Murtrie (1831). Such constant changes (altogether apart from the question whether barbarous names like the foregoing are admissible) are much to be deprecated; and the least an innovator can do is to make sure that he has got hold of the earliest name. Otherwise it is in every way far better to let matters stand as they are.

Mr. Palmer further urges that *Cyclothurus*, for the Pigmy Ant-eater, must give way to *Cyclopes*, Gray (1821); and, what is much worse, that *Uroleptes*, Wagler (1831), must replace its own name (*Tamandua*) for the Tamandua Ant-eater.

UNGER AND ENDLICHER.

Briefwechsel zwischen Franz Unger und Stephan Endlicher, herausgegeben und erlaüntert von G. Haberlandt. Nut Porträts und Nachbildungen zweier Briefe, pp. 184. Berlin: Borntraeger, 1899. Price 5 marks.

In this publication Prof. Haberlandt has made a most interesting contribution to the history of 19th century Botany. Unger and Endlicher were great men and great botanists, and this careful edition of their correspondence is full of instruction not unmixed with amusement.

A CARBONIFEROUS LANDSCAPE.

Eine Landschaft der Steinkohlen-Zeit. Erlaüterung zu der Wandtafel bearbeitet und herausgegeben im auftrage der Direction der Königl. Preuss. geologischen Landesanstalt und Bergakademie zu Berlin. By Dr. H. Potomé. Pp. 40, with 30 figs. and a plate. Leipzig: Borntraeger, 1899. Price with the "Tafel," 25 marks.

To restore the past is one of the most hazardous of tasks, and many have tried it with indifferent results. We have not as yet received the "Wandtafel" referred to above, but if it is in proportion to its size as good as the plate accompanying the pamphlet, it must be very good, for Dr. Potomé has put brains as well as artistic feeling into his picture. It is based upon plastic reconstructions of carboniferous plants, and seems to us so successful that we hope eagerly for more to follow.

L. ANTHROPOLOGIE, Tome x. No. 4.

L. Anthropologie for July and August contains some articles which will be of more than passing interest to those who are following the successive discoveries bearing on the prehistoric civilisation of Western Europe.

(1) Boule and A. Vernière (*L'Abri sous roche du Rond près Saint-Arcons-d'Allier* (*Haute Loire*)) describe the exploration of the rock-shelter of Rond, in the Auvergne district, which has yielded remains characteristic of the Reindeer period. Hitherto no stations of this description have been found in this part of France, at least that could be so dated from their relics. The station of Rond was situated under an overhanging cliff of the volcanic rock so common in the locality. Part of the accumulated *débris* had been previously removed, but sufficient remained to give an area of undisturbed strata of some 12 yards in length by 4 yards in breadth. At some depth in a talus of disintegrated rock and other materials the excavators came upon a black bed of ashes and organic matters, 8 inches thick, in which they discovered several hearths, some bone and flint implements, and osseous remains of various animals, including cave-hyena, reindeer, horse, stag, etc. Both the relics and the fauna are regarded by the authors as characteristic of the Reindeer period.

(2) Dr. Verneau (*Les nouvelles trouvailles de M. Abbo dans la Barma Grande*) recurs to the much debated age of the prehistoric men of Mentone, whose skeletons have, from time to time, been disinterred in the Baoussé-Roussé caves, near that town. Since 1892, when three skeletons were discovered in the Barma-Grande cave, two more have come to light in the same cave (1894), both, however, being at a depth of 5 feet less than the former. One of these skeletons—1·75 in. (about 5 feet 8½ in.) in height and strongly dolichocephalic—had associated with it a few ornaments of perforated teeth and shells. Thus in every respect it closely resembled the three burials discovered in 1892. The second, though only a few feet distant, showed evidence of having been subjected to great heat, as the bones were much carbonised. Dr. Verneau observes that the heat was applied to the body *in situ*, and that consequently it lay either on the surface of what was then the floor of the cave or in a very superficial trench. In the deposits beneath these skeletons portions of the lower jaw of a reindeer and some flint implements were found, which he assigns to the same chronological horizon as the human remains of the later Palaeolithic caves of France. The general conclusion arrived at is, that the two groups were contemporary, the three skeletons having been interred in deep pits in Palaeolithic *débris*, while the two upper ones were deposited at or near what was then the floor of the cave. On the whole he regards these

Mentone skeletons, with their associated relics, as approaching, in their general *facies*, more to Palaeolithic than to Neolithic civilisation.

(3) Salomon Reinach (*Un nouveau texte sur l'origine du commerce de l'étain*) combats the generally accepted opinion that, from the earliest times, the Phoenicians had a monopoly of the tin trade from the Cassiterides to the eastern shores of the Mediterranean until they were dispossessed of it by the Romans. He sets himself, with his usual facility in linguistic researches, to prove the following propositions: (1) that the Phoenician trade in tin has not been attested prior to the year 600 B.C.; (2) that the Phoenicians had not a monopoly of this commerce at any time; and (3) that the Greeks themselves never attributed to the Phoenicians, but to another people, the first commercial intercourse with the Cassiterides. His opinion is that the maritime commerce in tin was discovered by the barbarians of Western Europe, but only long after they became acquainted with the value of the metal, and the regions where it was to be found, through its transmission to the East by land routes. This view he considers to be in harmony with the archaeological evidence, which shows the diffusion of tin, amber, spiral ornaments, the types of bronze weapons and other objects, throughout the whole of Central and North-Western Europe during the Bronze Age.

We have to congratulate our contemporary, *La Feuille des Jeunes Naturalistes*, and the editor, Mr. Adrien Dollfus, on the fact that the November number begins the thirtieth year of the journal's existence. To Mr. Jean Dollfus thanks are due for his liberal assistance, which has made it possible to continue the modest price, and to form the valuable library which is at the disposal of the journal's readers. May *La Feuille* be evergreen, is our sincere wish!

Science for October 20 has an interesting article by Walter T. Swingle, U.S. Department of Agriculture, on the dioecism of the fig on its bearing upon caprification, a paper read before Section G of the American Association for the Advancement of Science at the Columbus meeting.

In the *Irish Naturalist* for October, Dr. Scharff describes an interesting variety of *Limax marginatus*, Müll. (var. nov. *niger*). Specimens were found during a preliminary survey of the MacGillicuddy's Reeks, at an altitude of 2500 to 3100 feet.

The October number of the *Journal of Conchology* contains, amongst other articles, a very useful synopsis of the American species of Diplodontidae, by Professor Dall, and an interesting paper by Mr. Edgar A. Smith, in which fourteen new species of South African marine shells are described and figured.

The Rev. A. H. Cooke contributes an important paper to the *Journal of Malacology* on the "Nomenclature of the British Nudibranchiata," to which is appended a revised classification of the group, based upon Bergh. In the same number Mr Henry Suter has an interesting paper on some New Zealand molluscs (*Paryphanta*, *Rhytida*, *Endodonta*, *Scalaria*, etc.), and Mr. J. Cosmo Melvill and Mr. Edgar A. Smith contribute illustrated papers describing new species.

The *Naturalist* for November contains, *inter alia*, articles on Lincolnshire Phalangidea, by Rev. E. A. Woodruffe-Peacock; on Lincolnshire Diptera, by the Rev. A. Thornley; on the modern tendency of mycological study, by Mr. Massee; and on the chemistry of the Lakeland trees, by Dr. Keegan.

The *Irish Naturalist* for November contains a long review of Dr. Scharff's "History of the European Fauna," by Mr. G. E. H. Barrett-Hamilton.

In the *Plant World*, No. 11, vol. ii. 1899, the first paper is by R. S. Williams—"Botanical Notes on the way to Dawson, Alaska." It describes

in an interesting itinerary the plants that came under observation. The wealth of mosses and lichens is noticed, and at Dawson city the prevalence of the Ericaceae and the scarcity of Compositae—features common to sub-alpine and sub-arctic situations.

A paper by L. H. Pammel, "Some Ecological Notes on the Muscatine Flora," is a study in hydrophytes, mesophytes, and xerophytes found in certain zones. A continued paper by Mrs. C. A. Creevey, "Plant Juices and their Commercial Values," gives a popular account of methods of extracting juices from plants in various parts of the world, and the physiological effects following the drinking of these juices.

In *Nature Notes* for November, Mr. A. E. Martin discusses editors and annotators of Gilbert White's "Selborne," Mr. F. Coleman discourses on birds and insects as meteorologists, Messrs. C. B. and C. T. Plowright describe Broadland in winter-time, and the Rev. George Henslow gives, for the benefit of young botanists, a beautifully clear statement of the evolutionist view of the origin of species. Naturally, he does not refrain from giving his own interpretation of the factors — the power to vary is called into action by new conditions, and the organs change in conformity or adaptation to these.

The *Westminster Review* for November, which we have received, is full of interesting matter, but the only article directly touching biological questions is a continued criticism of the Contagious Diseases Acts.

In the *American Journal of Science*, No. 44, vol. viii. August 1899, one paper is of interest to the biologist, namely, "Studies in the Cyperaceae," by Theo. Holm, and "On the abnormal development of some specimens of *Carex stipata*, Muhl., caused by *Livia vernalis*, Fitch" (with seven figures in the text drawn from nature by the author).

The diseased condition in question shows itself in the hypertrophied leaves, which become white, except at the tips, while they are flat from base to apex, and are devoid of the usual sheath. The larvae of the parasite were located on the upper surface of the leaves, and although the parasitism was purely superficial from its beginning to end, yet it resulted in the almost complete non-development of stomata, chlorophyll, lignin, and the partial non-absorption of silica. What sort of insect *Livia vernalis* is may be found in works on entomology—at least one would expect so,—for no light is thrown upon it in the article. Can any one suggest what advantage the author finds in using: mestome-bundle for fibro-vascular bundle, mestome-sheath for bundle-sheath, bark-parenchyma for cortex-parenchyma, perihadromatic bundle for—what? pericambium for pericycle, protohadrome for protoxylem, leptome for phloem? There is no need of bundle after mestome, which is equivalent to the whole term, fibro-vascular bundle. In roots one speaks of cortex-parenchyma, not bark-parenchyma. It is years since pericambium was given up for the better term, pericycle, because the form was apt to be confused with cambium.

We have just received from Dr. L. Bordas, Chef des Travaux Zoologiques in the Faculty of Science at Nancy, a paper from the fifth volume of the *Annales du Museé d'Histoire Naturelle de Marseille*, in which he shows, as we had previously occasion to note in "Fresh Facts," that the respiratory trees of Holothuroids have four functions—respiratory, hydrostatic, plastidogenetic, and excretory.

We have received the first part of Volume III. of the *Transactions and Proceedings of the Perthshire Society of Natural Science*, which contains the following papers :—"List of the Rhynchota of Perthshire," by T. M. M'Gregor and G. W. Kirkaldy; "The Flora of Durdie and Arnbathie," by James Menzies; "The Feathered Tenants of our Dwellings," by Lieut.-Col. W. H. M. Duthie; "On the Protection of Wild Birds in Perthshire," by Col. Campbell;

"A Naturalist's Notes on the Recent Voyage of the 'Blencathra' to the Arctic Regions," by William S. Bruce; "Notes on the Larch Disease," by Alex Pitcaithly. The society is now in its thirty-third year, and seems to be in a very healthy state. It is fortunate in having a splendid county to work in, a fine local museum, an indefatigable curator, and an enthusiastic president.

The Société Neuchateloise de Geographie has been good enough to send us its *Bulletin* (tome xi. 1899, pp. 320). It contains many instructive papers, *e.g.* on the "Préalpes Romandes," by Dr. H. Schardt; on "Persia," by Elisée Reclus; on "Esquimo Skulls," by Dr. Alex. Schenk; on "Skulls from the Valley of the Rhone," by Prof. E. Pitard. A clever geological map illustrates Dr. Schardt's paper.

The November number of the *American Journal of Science* has the following articles:—"Types of March Weather in the United States," by O. L. Fassig; "Some New Minerals from the Zinc Mines at Franklin, N.J., and Note Concerning the Chemical Composition of Ganomalite," by S. L. Penfield and C. H. Warren; "Action of Acetylene on Oxides of Copper," by F. A. Gooch and De F. Baldwin; "Andesites of the Aroostook Volcanic Area of Maine," by H. E. Gregory; "New Mode of Occurrence of Ruby in North Carolina," by J. W. Judd and W. E. Hadden, with crystallographic notes by J. H. Pratt. The scientific intelligence includes an obituary of the late Prof. Edward Orton.

The thirty-ninth publication of the Field Columbian Museum (No. 5, vol. i. of the botanical series) contains an account of *Higinbothamia*, a new genus of Dioscoreaceae, of other new forms in the same order, and of various new Amaranthaceae, by Dr. Edwin B. Uline.

The *Report and Transactions of the South-Eastern Union of Scientific Societies* for 1899 appears with admirable promptness. It contains the reports of various departments, the presidential address by Mr. W. Whitaker on the deep-seated geology of the Rochester district, and numerous papers of interest which we noted at the time of the annual meeting.

The October number of the *Journal of School Geography* contains *inter alia* an interesting article entitled "Life in the Grass Lands," in which a lively endeavour is made to relate human functions in the Steppes with the environmental conditions. The article is extracted from "Man and his Work: an Introduction to Human Geography," by Dr. A. J. Herbertson, of the Oxford Geographical School, and Mrs. F. D. Herbertson, B.A. The book should have been sent for review to *Natural Science.*

The *American Naturalist* for October has the following articles:—"Notes on European Museums," by O. C. Farrington; "On Some Changes in the Names of Fossil Fishes," by O. P. Hay; "The Utility of Phosphorescence in Deep-sea Animals" (to attract food), by C. C. Nutting; "A new Hydroid from Long Island Sound (*Stylactis hooperi*)," by C. P. Sigerfoos; "A Balloon-making Fly," by J. M. Aldrich and L. A. Turley; "Species of *Blissus* in North America," by F. M. Webster; and "Synopsis of North American Astacoid and Thalassinoid Crustacea," by J. S. Kingsley.

Among the articles in *Knowledge* for November we note "Shells as Ornaments, Implements, and Articles of Trade," by R. Lydekker; "Ups and Downs in our Daily Weight," by W. W. Wagstaff; and "Recent Work of the U.S. Biological Survey," by W. M. Webb.

The *Victorian Naturalist* for October contains *inter alia* a discussion of the question "Myxomycete or Mycetozoon?" by D. M'Alpine, and descriptions of some Australian birds' eggs by D. Le Souef.

Among the articles in the *Zoologist* for November there is an interesting diary by Edmond Selous concerning the habits of nightjars, and an account by J. L. Monk of the spawning of *Bombinator pachypus* after two years of captivity in England.

Science Gossip for November contains, among other articles, the following:— "On colouring of Birds' Eggs," by R. J. Hughes, and "On Armature of Helicoid Land-shells," by G. K. Gude. There is also on p. 191 a suggestion well worthy of consideration in regard to co-operative science collections.

The *Journal of the Institute of Jamaica* (vol. ii. No. 6, issued 31st August 1899) contains much interesting matter, a large number of short historical articles, *e.g.* "The Story of the Life of Columbus and the Discovery of Jamaica," by the editor Mr. Frank Cundall, and many scientific papers, of which those by Mr. J. E. Duerden, the enthusiastic and indefatigable curator of the museum, may be especially noted.

Rhodora for November has among its articles one on adventitious plants of *Drosera*, by R. G. Leavitt, and one on the white blackberry, by A. M. Mitchell.

We have also received the following:—

On the physiological perception of musical tone. Being the seventh Robert Boyle Lecture delivered before the Oxford University Junior Scientific Club on 6th June 1899. By Prof. John Gray M'Kendrick, M.D.; LL.D.; F.R.SS. L. and E. Pp. 65. London: Henry Frowde, 1899. Price one shilling net.

A continuation of Acloque's "Faune de France," dealing with birds (pp. 87-336, with 621 figures. Paris: Baillière, 1899, price 5 francs), which sustains the reputation of the previous volumes.

Also a paper by Mr. A. C. Seward, previously noticed in our pages, "On the Structure and Affinities of *Matonia pectinata*, R. Br., with notes on the geological history of the Matonineae," *Phil. Trans.* Series B, vol. cxci. 1899. Pp. 171-209, 4 pls. Price 4s. 6d.

The first Lancashire Sea-Fisheries memoir, "Oysters and Disease, an account of certain observations upon the normal and pathological histology and bacteriology of the oyster and other shell-fish," by Profs. W. A. Herdman and R. Boyce. 4to, pp. 60, 8 pls. London: Philip and Son, 1899. Price 7s. 6d. net.

"The Concilium Bibliographicum in Zürich and its work," by W. E. Hoyle, M.A., and Clara Nördlinger of the Manchester Museum (which is proud to be the possessor of the only complete set of the Zürich cards in England). The paper is reprinted for private distribution from the *Library Association Record*, November 1899; and it is hoped that it may do something to promote increased appreciation of Dr. Field's self-sacrificing bibliographic energy.

"Notes on the Binney Collection of Coal-measure Plants," by A. C. Seward, M.A. Part I. deals with *Lepidophloios*, and Part II. with *Megaloxylon* gen. nov. *Proc. Cambridge Philos. Soc.* x. 1899, pp. 137-174, 2 pls. and 5 figs.

OBITUARIES.

The following deaths have been recently announced :—Dr. OSCAR BAUMANN, the African explorer, at Vienna, on October 12; EDWARD CASE, on September 22, an English engineer well known for his method of groyning to prevent encroachments of the sea on the coast; CORNELIO DESINIONI, on June 29, in Gavi, Italy, a historian of geography, in his 86th year; on September 19, in Poturzyca (Galicia), in his 72nd year, GRAF WLADIMIR DZIEDUSZYCKI, curator and founder of his Natural History Museum in Lemberg, which is especially rich in birds; Prof. H. R. GEIGER, sometime assistant on the U.S. Geological Survey, at Springfield, Ohio, July 18; on July 2, in Regensburg, WILHELM GEYER, a well-known enthusiast on aquaria; on July 16, NIKOLAUS W. GRIGORJEW, a young phyto-palaeontologist, in Charkow; Dr. RAGNAR HULT, geographer and botanist, at Helsingfors, in his 42nd year; PAUL JANET, the illustrious professor of philosophy at the Sorbonne; on July 2, in Pará, the botanist, Dr. F. KUHLA, about to start on a botanical expedition to the tributaries of the Upper Amazon; on June 30, in Stockholm, in his 78th year, Dr. MATTS ADOLF LINDBLAD, for twenty years docent in botany in the University of Upsala, known as a mycologist; in Budapest, in his 55th year, GÉZA VON MIHÁLKOVICS, the famous anatomist, professor of anatomy and embryology in the University of Budapest; on October 16, Dr. EDWARD ORTON, geologist, professor in the Ohio State University, president of the American Association for the Advancement of Science; on August 9, Mr. WILLIAM PAMPLIN, in his 93rd year, the *doyen* of English botanists, who contributed largely to the "London Catalogue of British Plants"; Lady PRESTWICH, who recently published a biography of her husband, at Parkstone, on August 26, at the age of 66; on August 2, in Buenos Aires, in his 72nd year, GEORG RUSCHEWEYH, a keen lepidopterist; in Scutari (Albania) the ornithologist GEORG FREIHERR SCHILLING VON CANSTATT; on October 21, JAMES SIMPSON, for eighteen years curator of the Anatomical Museum, University of Edinburgh; W. A. SNOW, late instructor in entomology in Stanford University, drowned on October 10, in San Francisco harbour; on August 3, in Lucerne, SIEGFRIED STAUFFER, founder of the Natural History Museum there; in September, Dr. CARL GUSTAF THOMSON, curator of the entomological department of the zoological museum in Lund, an authority on Hymenoptera; at Lakefield (Ontario), Mrs. C. P. TRAILL, botanist, in her 97th year; on September 9, in Pöls (Steiermark), the ornithologist Dr. STEPHAN FREIHERR VON WASHINGTON, in his 41st year; Dr. HENRY HICKS, F.R.S., the distinguished geologist, on November 18, at the age of sixty-two.

CORRESPONDENCE.

A PORTUGUESE PARALLEL TO *NEOMYLODON LISTAI.*

If a mouse may help a lion, may I venture to draw attention to the rather striking parallelism which exists between the discovery of such fresh remains of *Neomylodon* in the dust of a large cavern near Lost Hope Inlet, and Dr. Gadow's find of several skeletons of the Norway Lemming (*Lemmus lemmus*) near Athouguia in Portugal.

Through the kindness of Dr. Gadow I was permitted to announce the discovery at the meeting of the Zoological Society of March 3, 1896 (see *P. Z. S.*, March 6, 1896, pp. 304-306). The circumstances of the case are quite close to those attending the discovery of *Neomylodon.* In both cases the bones were discovered buried under the dust of a cave, in both cases they were surprisingly fresh (the Lemming remains were quite recent, having the skin and the ligaments attached to them), and in both cases the remains found are those of an animal believed to have been long since extinct in the country where they were found.

The present range of the Norway Lemming does not extend south of about 58° 30′ north latitude, while even in Pleistocene times it had been previously unknown from any localities south of England, yet its remains as found in Portugal had the appearance of having belonged to quite recently dead animals.

It would seem then that even in countries where the climate is damp, or certainly not dry, it is possible that, given the aid of a sheltering cave, and of abundance of dry dust, the remains of mammals, both small and large, may be preserved in quite a fresh state for long periods.

G. E. H. Barrett-Hamilton.

Kilmanock, Arthurstown,
Ireland.

BIOLOGICAL ANALOGY AND SPEECH-DEVELOPMENT.

As language (speech) is entirely a human invention—just as chess and piano-playing are—the science of language is not entitled to be classed as a natural science; so it is with much diffidence that I write to you on the subject. But as you published Mr. Henry Cecil Wyld's paper on "Biological Analogy and Speech-development" in your January (1899) Number, may I venture to point out to Mr. Wyld that in his criticism of the fallacy of Professor Paul's reasoning he might possibly mislead as many readers as Professor Paul has.

Mr. Wyld says (p. 48) that "the safest way to think of language is as a habit of body expressing a habit of mind."

The question naturally arises, "Is this a safe way to think of any human invention?"

One might just as well say that the safest way to think of bicycle-riding is as a habit of body expressing a habit of mind, though the cerebration is unconscious—in the case of good riders—just as it is with accomplished speakers in language.

But wherein comes the element of safety pointed out by Mr. Wyld? I should prefer to say that Mr. Wyld's way of thinking of language is a very vague one, and vagueness of thought is not an element of safety in scientific inquiry.

It seems to me far safer for writers on the subject never to lose sight of the fact that language (speech) is a human invention, and has nothing whatever to do with biological analogy or biological phenomena.

Then we shall probably hear a great deal less of the "life" and "growth" of language, its "evolution," its "branches," its "offshoots"; that it is an "organism," that it has "roots," and that there are "mother-languages" and "sister-languages"; and all the rest of the jargon with which philologists becloud their subject.

Philologists will retort that these terms are merely metaphorical: but these metaphors mislead, and have misled many who read books on philology to get a knowledge of what language is. J. I. HAZELAND.

KOBE CLUB, KOBE, JAPAN,
Sept. 6, 1899.

NEW MEXICO BIOL. STATION.

Your note on p. 157 about the N. M. Biol. Station is incorrect. The Biol. Station was conducted by myself and Miss Wilmatte Porter, and concerned itself not at all with geology or anthropology. The students were mostly public school teachers, and occupied themselves with the biology of flowers, particularly the structure of flowers as related to insect visitors. Some work was also done on the mouth-parts of bees, and a few other things. It seemed to me we had as much success as we deserved, and the outlook for the future is encouraging. It is regretted that there is no millionaire available to endow the institution; but the country is full of new and interesting things, and is itself a laboratory better endowed than that of many a wealthy college, so that the naturalist who cannot find profitable occupation must be stupid indeed. The station differs from most others in concerning itself with terrestrial life (not freshwater, or marine), which is especially worth the attention of the student in this region, owing to the desert conditions, resulting in such interesting adaptations.

Your notes on Dr. Judd's paper (p. 89) are interesting. Yesterday I saw a little spider which beautifully mimicked an ant of the genus *Formica*. Now you might say, what for? The ant is a fairly soft, harmless thing, apparently as good meat as the spider. But the great enemy of spiders is a certain wasp, which stores up spiders for its young. Now the wasp *doesn't want ants*, doesn't use that kind of meat. So the spider taken for an ant will escape, though the ant *is* harmless. This couldn't be seen on general principles, one has to know about the customs of the wasp. THEO. D. A. COCKERELL.

MESILLA PARK, NEW MEXICO, U.S.A.

NEWS.

THE following appointments have recently been made :—Dr. Hugo Berger, to be professor of the history of "Erdkunde" in Leipzig; Dr. Edgar R. Cummings, as instructor in geology in the University of Indiana, Bloomington; Dr. E. A. Darling, as bacteriologist to the Cambridge Board of Health, to succeed Dr. G. B. Henshaw; Dr. C. B. Davenport, to fill the post in the University of Chicago left vacant by the removal of Professor Wheeler to the University of Texas; W. L. H. Duckworth, M.A., as lecturer in physical anthropology at Cambridge University; O. Franges, to be professor of pisciculture at the University of Agram; Dr. Sigmund Fuchs, as professor of the anatomy and physiology of domestic animals at the Agricultural Station at Vienna; Dr. K. W. Genthe, as an instructor in zoology in the University of Michigan; Dr. L. C. Glen, as professor of geology in South Carolina College; Dr. Hans Hausrath, to be professor of forestry in the Technical Institute of Karlsruhe; Dr. Henneberg, as docent in anatomy at Giessen; Dr. L. Hiltner, as director of the bacteriological laboratory in the Imperial Health Office in Berlin; V. Hlavinka, as professor of geodesy in the University of Agram; Dr. S. J. Holmes, as an instructor in zoology in the University of Michigan; Dr. E. Jacky, as assistant on the botanical side of the pomological institute in Proskau; J. J. Jahn, to be professor of mineralogy and geology in the Technical Institute in Brunn; Dr. H. S. Jennings, as an instructor in zoology in the University of Michigan; Dr. Stefan Jentys, as professor of agriculture and botany in the University of Agram; Dr. Johannes Christoph Klinge, to be a head botanist and the librarian in the botanic garden of St. Petersburg; S. J. Korshinsky, to be director of the herbarium of the Academy of St. Petersburg; Dr. Alfred Krolopp, as assistant professor of botany in the University of Agram; Dr. Daniel P. MacMillan, to an appointment in connection with the Child-Study Department recently created in connection with the public schools of Chicago—probably the first appointment of this sort, and, we sincerely hope, not the last; Dr. W. D. Merill, as instructor in biology, with special reference to botany, in the University of Rochester; Dr. Merton L. Miller, associate in anthropology in the University of Chicago; Dr. B. Němec, as docent in vegetable anatomy and physiology in the Tschech University of Prag; W. A. Orton, as lecturer on botany in the St. Louis Manual Training School in New York; C. W. Prentiss, as an assistant in zoology at Harvard University; Dr. Eugen Romer, as docent for geography in the University of Lemberg; Dr. J. T. Rothrock, reappointed state commissioner of forestry for the state of Pennsylvania; John Louis Sheldon, as assistant in botany in the University of Nebraska in Lincoln; Dr. E. O. Sisson, as director of the histological laboratory in the recently consolidated medical schools of Keoduk, Iowa; G. Tanfiljew, to be a head botanist in the botanical institute in St. Petersburg; Mr. J. L. Tuckett, Fellow of Trinity College, Cambridge, as an additional demonstrator of physiology; Dr. Velich, as docent in the physiology and pathology of animals in the Tschek University of Prag;

Nikolaus Warpachowsky, as director of the Government Fisheries in Archangel; Dr. Karl Wenle, as docent in geography and ethnology in the University of Leipzig; W. A. Willard, as an assistant in zoology in Harvard University; S. R. Williams, as an assistant in zoology in Harvard University.

Dr. K. Eckhardt, Professor of Physiology at Giessen, has recently celebrated the fiftieth year of his function as a university teacher.

Professor Henry G. Jessup, who has held the chair of Botany in Dartmouth College for twenty-two years, has resigned.

The Council of the Royal Society has adjudicated a Royal medal to Professor William Carmichael M'Intosh for his important monographs on marine animals, his work on the fisheries industries, and his success in establishing the Gatty Marine Laboratory at St. Andrews.

The Council of the Royal Society has adjudicated the Davy medal to Mr. Edward Schunck, F.R.S., for his investigations on madder, indigo, and chlorophyll.

The gold medal of the Highland and Agricultural Society of Scotland has been awarded to Professor Cossart Ewart in recognition of his experiments on hybridisation, telegony, and the like.

Mr. J. J. Lister, University Demonstrator of Comparative Anatomy, and Mr. A. C. Seward, University Lecturer in Botany, have been elected to fellowships in St. John's College, Cambridge, in recognition of their important scientific work.

Dr. G. Elliott-Smith, one of the assistant demonstrators of Anatomy at Cambridge, well known for his researches on the comparative anatomy of the mammalian brain, has been elected a Fellow of St. John's College.

Professor G. Sims Woodhead has been elected to a Fellowship at Trinity Hall, Cambridge.

The degree of M.A. *honoris causa* has been conferred by the University of Cambridge on Dr. W. Somerville, recently elected Professor of Agriculture there.

Grants from the Moray fund of the University of Edinburgh have been made to Professor E. A. Schäfer for the expenses of research on the cerebral nervous system, and to Dr. John Malcolm for experiments on the alterations in bone marrow produced by nucleins and their allies.

At the unveiling of the monument to Johannes Müller, at his birthplace, Coblentz, on October 2nd, Professors Virchow and Waldeyer were the chief speakers. The former pointed out that Johannes Müller was *par excellence* a biologist; the latter referred especially to Müller's influence on the University of Berlin, and on the Prussian Academy of Sciences.

The following gifts and bequests are announced:—D. F. Converse, a mill-owner of Spartanburg, S.C., left one-third of his estate, valued at half a million dollars, to Converse College, an institute which he founded ten years ago in Spartanburg for the higher education of women; by the will of the late Cornelius Vanderbilt, Yale University receives $100,000, and Vanderbilt University half that sum; £20,000 given by Mr. Charles Holcroft for the new Birmingham University, bringing the total endowment up to £315,400; £10,000 was recently subscribed towards enlarging the Durham University College of Science, for which £50,000 is needed.

Vassar College has been promised $25,000 towards a biological laboratory on condition that an equal amount be raised otherwise.

Mr. E. E. M'Millin has given the Ohio Academy $250 for scientific investigations, with a provisional promise that the gift may be annual.

Mr. E. Tuck has given $300,000 to Dartmouth College, U.S.A.; the late Mrs. M. J. Goddard left $60,000 to Tufts College.

The list continues:—$140,000 left by Dr. Calvin Ellis, formerly Dean of the Harvard Medical School, to the University; $90,000 bequeathed by Miss Lucy Ellis, to be added to the fund left by her brother, Dr. Ellis; $50,000 given by an anonymous donor to the University of Pennsylvania for the dormitory system; $25,000 bequeathed to Wesleyan University, Middletown, Conn., by J. H. Sessions; $10,000 given to the Iowa Wesleyan University by ex-Senator James Harlan.

We learn from *Science* that a large collection of water-colour paintings of Japanese fishes by a Japanese artist has been presented to the University of Michigan by Frederick Stears, of Detroit, and is at present on exhibition in the University Museum.

Prof. Starr of Chicago has presented his collection illustrating the ethnography of Mexico to the Folk Lore Society, who have offered to deposit it in the Museum of Archaeology and Ethnology at Cambridge.

£1000 has been bequeathed by the late Mr. C. P. Daly to the American Geographical Society for the foundation of a medal to be awarded for distinguished services in geography.

The *American Naturalist* notes that the sons of the late Prof. J. Marcou have presented his geological library to the American Museum of Natural History in New York.

Over fifty students, says the *American Naturalist*, attended the Coldspring Harbour biological laboratory during the summer of this year.

It is noted in *Science* that the expenses of the University of Chicago for printing and publishing during the academic year ending June 30, 1899, were over $44,000, while the receipts were only $17,000. It is probable that no other University supports its publications with such liberality.

It is stated in the *Scientific American* that the number of women in attendance at the German Universities during the summer semester of 1899 was 355. There were 179 at Berlin, 45 at Bonn, 27 at Breslau, 29 at Göttingen, 13 at Heidelberg, and 19 at Halle. The University at Strasburg has just decided to admit women to its courses. Hitherto it has closed its doors to women, but now there is no German university where they may not pursue their studies.

There are fifteen Universities in France, with 27,080 students, of whom 12,059 belong to Paris. The total expenditure is 13,859,500 francs, of which 10,524,200 has each year to be found by the State.

The *Scientific American* notes that last year the regents of the University of California sent out invitations to the architects of Europe and the United States to participate in a competition whose object was to secure the best possible plans for new buildings for the university. A careful programme was outlined, and in deference to European architects, Antwerp was selected as the city where the first competition should be held, and 101 plans were received from architects in every country in Europe and from the United States as well. A representative international jury passed on the plans.

On September 8 they announced that the plan of M. E. Bénard, of Paris, was successful and would receive the $10,000 prize. Mrs. Phoebe A. Hearst gave $100,000 for defraying the necessary expense of the competition; she has also promised to bear the cost of some of the buildings. The whole scheme calls for $20,000,000.

It is good news that the Liverpool Marine Biology Committee has published the first of a series of Memoirs on typical British marine plants and animals, edited by W. A. Herdman, D.Sc., F.R.S. No. 1 is on *Ascidia*, by Professor W. A. Herdman, D.Sc., F.R.S. It has 60 pp. and 5 plates, and costs 1s. 6d.

It is hoped that this series of special studies, written by those who are

thoroughly familiar with the forms of which they treat, will be found of value by students of Biology in our laboratories and in marine stations, and will be welcomed by many others working privately at marine natural history.

It is proposed that the forms selected should, as far as possible, be common Irish Sea animals and plants, of which no adequate account already exists in any text-book.

The first three Memoirs will be issued before the end of 1899, and others will follow, it is hoped, in rapid succession :—Memoir I. *Ascidia*, W. A. Herdman ; Memoir II. Cockle, J. Johnstone ; Memoir III. *Echinus*, H. C. Chadwick ; *Dendronotus*, J. A. Clubb ; *Zostera*, R. J. Harvey Gibson ; *Halidrys*, C. E. Jones ; *Codium*, R. J. H. Gibson and Helen Auld ; Diatoms, F. E. Weiss ; *Gigartina*, O. V. Darbishire ; *Alcyonium*, S. J. Hickson ; Plaice, F. J. Cole and J. Johnstone ; *Botrylloides*, W. A. Herdman ; Cuttle-fish, W. E. Hoyle ; Ostracod, Andrew Scott ; *Patella*, J. R. Ainsworth Davis ; *Calanus*, I. C. Thompson ; *Actinia*, J. A. Clubb ; Polyzoon, Laura R. Thornely ; Calcareous Sponge, R. Hanitsch ; Porpoise, A. M. Paterson ; *Arenicola*, J. H. Ashworth ; Oyster, W. A. Herdman.

The editor acknowledges a welcome donation of £100 from Mr. F. H. Gossage of Woolton, which has met the expense of preparing the plates in illustration of the first few memoirs, and so has enabled the Committee to commence the publication of the series sooner than would otherwise have been possible.

The Committee desire to intimate that no copies of these memoirs will be presented or exchanged, as the prices have been fixed so low that most of the copies will have to be sold to meet the cost of production.

The memoirs may be obtained, post free at the net prices stated, from the Hon. Treasurer, Mr. I. C. Thompson, 53 Croxteth Road, Liverpool ; Professor Herdman, University College, Liverpool ; or the Curator, Biological Station, Port Erin, Isle of Man.

The Millport Marine Biological Station issues an appeal for a sum of £300, required for the pumping and circulating apparatus. The fund for this is to be kept independent of the general maintenance accounts. The Millport Marine Station has the distinction of being a scientific institution founded and maintained by private liberality on the part of persons interested in the advancement of science, and it will be a matter for congratulation if, before the Glasgow meeting of the British Association in 1901, its equipment is complete in the important department to which this appeal has special reference.

We read in *Science* that teachers in Philadelphia public schools are now allowed to take their classes for a half-day once or twice a year to the Zoological Gardens and Fairmount Park, the visit counting as part of the regular class duties.

Science reports some of the general results of the third Princeton expedition to Patagonia, conducted by Mr. J. B. Hatcher and his assistant Mr. O. A. Paterson.

(1) A good preliminary geological survey of that part of southern South America lying between the Andes on the west and the Atlantic on the east, and between the Straits of Magellan and the forty-seventh parallel of south latitude, sufficient to serve as a basis for a geological map of the region.

(2) Very extensive and complete collections of fossils from all the horizons known to that region, with the exception of the *Pyrotherium* beds.

(3) The discovery of four distinct and previously unreported geological horizons.

(4) A collection of more than a thousand skins and skeletons of recent birds and mammals.

(5) Extensive collections of the freshwater, terrestrial, and littoral invertebrates.

(6) Botanical collections, especially of the mosses, hepaticae, and flowering plants, not including the grasses and sedges.

(7) A large series of photographs illustrating the geology and physical geography of Patagonia.

The geology will be treated of by Mr. Hatcher, the Tertiary invertebrates by Dr. Ortmann, the fossil vertebrates by Messrs. W. B. Scott and Hatcher, and the recent birds by Mr. W. E. D. Scott.

At the meeting of the Biological Section of the New York Academy of Sciences on October 9, Professor H. F. Osborn gave an account of the exploration of the American Museum party in Southern Wyoming, which resulted in the discovery of Dinosaur remains; Professor E. B. Wilson reported the discovery of females of *Polypterus* in Egypt, but with unripe ovaries, and the rediscovery of the branchiate Oligochaete *Alma;* and Professor Dean reported finding on the Californian coast freshly hatched young of *Bdellostoma*, and many stages of *Chimaera collieri.*

Professor Franz von Höhnel of Vienna has undertaken a botanical exploration in Brazil.

In *Nature* for November 9 Mr. John C. Willis gives an account of the facilities now available in Ceylon for botanical research.

We learn from the *Scientific American* that the Duke of Abruzzi has found an important mistake in the last map of Franz Josef Land. He says that Cape Flora is really ten geographical miles east of the post assigned on Jackson's map. The map of Payer was riddled by Jackson, who complained of its inaccuracies, but he has himself assigned the wrong position to his own camp.

On the Skeat expedition Mr. Evans found several species of *Peripatus* in Kalantan. As the distribution of this animal is of peculiar interest we may note also that in 1886 Mr. R. Horst recorded its occurrence from East Sumatra on the other side of the Malaka Strait. See *Nature*, November 9, 1899, p. 31.

Science reports that Mr. R. E. Snodgrass, assistant in entomology in Stanford University, and Mr. A. H. Heller, have returned from a successful ten months' collecting trip to the Galapagos Islands. The collections of birds, fishes, insects, and spiders, are said to be large.

In the judgment of Major Ronald Ross, who has now returned from Africa, the future of the west coast will be assured as soon as the colonial authorities take steps similar to those now in operation in Sierra Leone, to destroy the virulent mosquito.

Geheimrath Prof. von Zittel of München is arranging to send a scientific expedition to Patagonia.

We learn from *Science* that Mr. O. F. Cook of the Division of Botany, U.S. Department of Agriculture, has been sent to examine the plant products of Puerto Rico in reference to the possibility of introducing new and useful tropical plants into the island. He is accompanied by Mr. G. N. Collins as photographer, and Mr. G. P. Gall sent by the Smithsonian Institution to collect material for the National Herbarium.

Nature reports that another British exploring expedition to Abyssinia has been arranged, and will leave England at once for nine months. The objects are science and sport.

The annual conversazione of the Geologists' Association, London, was held on November 3, and was fairly well attended in spite of the inclement weather. Among the more striking exhibits were a fine series of concretionary structures brought together by Dr. G. Abbott; the skin and skull of *Neomylodon listai* lent by the La Plata Museum, and shown by A. Smith Woodward; a series of pebbles from Derbyshire compared with a corresponding series from

the London basin, and taken by Mr. A. E. Salter as evidence for a former river-connection between the two areas. From Derbyshire also, as a result of the long excursion, came a collection of Carboniferous limestone fossils made by Miss M. C. Foley, as well as various photographs. The Carboniferous limestone of the Isle of Man had yielded to Miss C. Birley a good set of Cephalopods. W. H. Chadwick and P. Emary showed Graptolites from the Wenlock shales and Llandeilo beds of Builth and St. David's. English and Indian Trigonias were shown by Prof. J. F. Blake, and other fossil collections by H. W. Burrows, W. F. Gwinnell, and F. R. B. Williams. The last mentioned also exhibited William Smith's Geological Sections from London to Snowdon. In contrast the latest maps of the Geological Survey were shown by Sir Archibald Geikie. A. S. Foord exhibited photographs of the striking frescoes in the Historical Museum at Moscow, showing scenes of Russian life in the Stone Age and in the tenth century. Wind-worn pebbles from England, Esthland, New Zealand, Bohemia, and Egypt were shown by F. A. Bather and Rev. Prof. T. G. Bonney, the latter also sending schistose Jurassic rocks from Nufenen and Scopi in the Alps, and Pre-triassic Alpine Schists from the Val Piora. These and many other exhibitors showed that the activity of the Association was in no way diminishing.

Mr. P. L. Sclater, on his recent visit to South Africa, gave an address to the South African Philosophical Society, in which he pointed out the desirability of establishing a Zoological Garden in Cape Town. It was doubtless towards this end that Mr. Rhodes sent his lion.

The lectures to be delivered before the Hull Scientific and Field Naturalists' Club during the rest of the winter session, 1899-1900, include the following:—"Natural History Notes in North Wales," by the President, R. H. Philip; "Symbiosis—A study in Plant Partnerships," by Mr. J. E. Robinson; "Cyclone and Cloud—A study of English Weather," by Mr. C. H. Gore, M.A.; "Solar Eclipses, with special reference to that of May 28, 1900," by Rev. H. P. Slade; "Wild Fowling and Decoying," by Mr. T. Audas, L.D.S.; "Economical Illumination," by Dr. J. T. Riley, A.R.C.Sc.I.; "What is a Species?" by Dr. H. H. Corbett, M.R.C.S. (of Doncaster). In January the club will hold an exhibition and conversazione.

The *Scientific American* notes that "it is not often that specimens in museums are destroyed by reason of being eaten, but it seems that in one of the Southern States a negro clay-eater who was employed as a scrubwoman devoured some of the finest specimens of kaolin on exhibition at the State Geological Museum. The State geologist found that five blocks of clay which were very highly valued on account of their purity were missing, and upon examining some of the other specimens he found on them the impression of teeth. Detectives were set to work on the case, and the negress employed to scrub the marble floors was accused of taking the specimens. The woman appears to have a mania for eating clay, and she had been indulging her strange appetite for some time."

Knowledge notes that a collection illustrating changes due to domestication has been begun at the British Museum (Natural History). A number of interesting stuffed specimens and skeletons have been placed on exhibition in the gallery of British Zoology.

The *Scientific American* notes that the city of New York has made an appropriation of $10,000 for the purpose of making a great relief map of the whole city. The map will be about 50 feet square, and will show all the important buildings. Buffalo will also be represented in probably the same manner, with a relief map which will show Niagara Falls and its power plants.

We learn from the *Scientific American* that the U.S. Department of Agriculture desires an ornithological clerk who must have an excellent knowledge of orni-

thology and mammalogy, and his examination will include a practical test in the identification of specimens of birds and mammals. In fact, these two subjects count 70 per cent in the examination to be held. The person who succeeds in passing will be placed on the eligible list, and if selected will receive the munificent salary of $660 per annum.

The *American Naturalist* notes that an Entomological Society has been founded, with Dr. E. F. Felt, State Entomologist, as president.

The *Scientific American* notes that the executors of the late Prof. O. C. Marsh have sold his valuable collection of orchids, but the prices were extremely low. It seems a pity that a collection of this size and importance was not procured intact for some botanical garden.

Prof. A. L. Herrera has been kind enough to send us a small sample of calcareous soap mixed with albumen and peptone, which when warmed on the slide with water will move and fill with vacuoles, without, however, giving off any pseudopodia.

We learn from the *Scientific American* that for several years attempts have been made at Omaha and Los Angeles to hatch the eggs of the ostrich artificially, but so far we believe their attempts have been unsuccessful, the difficulty being the application of moisture. Now, however, an ostrich farm in Florida can boast of the first incubator-hatched ostrich in the United States. The incubation required forty-one days of careful watching, the thermometer was kept at 110° and the moisture was applied at intervals.

On November 21 the Edinburgh Town Council gave a favourable reception to an influential deputation who appeared in order to urge the Corporation to give their influence towards the promotion of the movement for the establishment of a zoological garden in Edinburgh.

The Mortimer Museum of Antiquities at Driffield, Yorkshire, contains a very good local collection. Its owner has offered it to the East Riding County Council for half its value, the value to be decided by two referees, one to be appointed by the Council and the other by Mr. Mortimer. We understand that the Council has, on legal grounds, some hesitation in accepting this generous offer; but we hope that it will be bold enough to follow the example of other County Councils, as otherwise, on Mr. Mortimer's death, the collections will be sold and scattered.

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